Journal
of
The Franklin Institute
Devoted to Science and the Mechanic Arts

Vol. 186 JULY, 1918 No. 1

SCIENTIFIC RESEARCH IN THE CANNING INDUSTRY.*

BY

W. D. BIGELOW, Ph.D.,
Chief Chemist, National Canners' Association.

In September, 1913, Dr. A. D. Little delivered the annual address as President of the American Chemical Society. His subject was "Industrial Research in America," and his address began with the following words:

"Germany has long been recognized as preëminently the country of organized research. The spirit of research is there imminent throughout the entire social structure. This is not the time or place, however, nor is it necessary before this audience, to refer in any detail to the long record of splendid achievement made by German research during the last fifty years. It is inscribed in luminous letters around the rock upon which Germany now stands secure among the nations of the world.

"The virility and range of German research were never greater than they are to-day. Never before have the superb energy and calculated audacity of German technical directors and German financiers transformed so quickly and so surely the triumphs of the laboratory into industrial conquests. Never has the future held richer promise or orderly and sustained progress, and yet the preëminence of Germany in industrial research is by

* Presented at a meeting of the Section of Physics and Chemistry held Thursday, March 7, 1918.

[Note.—The Franklin Institute is not responsible for the statements and opinions advanced by contributors to the JOURNAL.]

COPYRIGHT, 1918, by THE FRANKLIN INSTITUTE.

no means indefinitely assured. A new competitor is even now girding up his loins and training for the race, and that competitor is, strangely enough, the United States—that prodigal among nations, still justly stigmatized as the most wasteful, careless, and improvident of them all."

He then discussed briefly some of the more striking illustrations of industrial research in this country. As we listened to this comprehensive survey we little realized the transformation that was so soon to begin in the field of industrial research throughout the world.

In the late summer of 1914 Europe was suddenly convulsed with the present war. Instead of mere armies being pitted against each other, whole nations, with all their resources, were involved. It soon became evident that the long development of the German nation in industrial research gave that country an important advantage which could only be offset by a systematic organization of the scientific resources of the countries opposing her. Unfortunately, in the early days of the war, the scientific men of England and France were sent to the trenches, where many of them were sacrificed. Steps were finally taken to restore to the laboratories those who were left, and industrial research was organized on a scale which had not been contemplated before.

Within the first few months of the war Great Britain decided that the system of industrial research of the nature found necessary in war time should be extended and placed on a permanent basis, and an organization was effected looking to that end. On July 28, 1915, approximately eleven months after the beginning of the war, a committee of the Privy Council was appointed to direct the application of any sums of money provided by Parliament for the organization and development of scientific and industrial research. An Advisory Council was also appointed, and to it were referred recommendations:

(I) For instituting specific researches;

(II) For establishing or developing special institutions or departments of existing institutions for the scientific study of problems affecting particular industries and trades, and

(III) For the establishment and award of research studentships and fellowships.

It is especially noteworthy that the Advisory Council did not

limit the scope of its plan to industries necessary for the prosecution of the war. Not only was it concerned with research as supported by the government or conducted by educational institutions, but also with encouraging coöperative industrial research conducted partly or entirely by manufacturing and trade organizations.

The Advisory Council has issued two reports which show that the work has been surveyed on a scale and in a manner that must necessarily exert a sweeping influence on manufacturing industries. The overseas dominions, including Canada, Australia, South Africa, and New Zealand, have appointed appropriate councils and taken action in line with the movement of the Advisory Council of Great Britain.

It is obvious that systematic coöperation of this kind must necessarily advance the manufacturing industries and the public welfare. The movement marks a new epoch in the world's history. On this subject the Advisory Council says:

"It will inevitably tend to bring industries into intimate relation, which are at present independent of each other; to transform what have hitherto been crafts into scientific industries, and to require coöperation not only between different firms in the same industry, but between groups of industries in a continuously widening series of interrelated trades."

Active coöperative research in connection with the manufacturing departments of corporations that have known each other only as rivals represents a wide departure from our customary habits of thought. Under ordinary conditions so important a step would be taken only after great deliberation. The progress made, therefore, may be attributed to the patriotism of British manufacturers.

Even in the first report of the Advisory Council it is stated that

"There are indications of a change of view among certain firms which give us hope that a more far-reaching coöperation may be possible, and we understand that the necessities of this war have led to an exchange of information once tenaciously reserved, which may survive the present critical days and, if it does, will augur well for the future."

The same report also says:

"The necessity for the central control of our machinery of

war had been obvious for centuries, but the essential unity of the knowledge which supports both the military and industrial efforts of the country was not generally understood until the present war revealed it in so many directions as to bring it home to all. War has remained as much an art as ever, but its instruments, originally the work of the craftsman and the artist, are now not only forged by the man of science; they need a scientific training for their effective use. This is equally true of the weapons of industry. The brains, even the very processes, that to-day are necessary to the output of munitions were yesterday needed, and will be needed again to-morrow, for the arts of peace."

It is now generally recognized that at the beginning of the war scientific research in the United States was better organized than in any other country, with the exception of Germany. During the last three years scientific research in the United States has increased by leaps and bounds. Great Britain, however, has far outstripped us. Her early participation in the war necessitated the utmost activity in scientific research. Her systematic encouragement of coöperation in this field of labor has resulted in unprecedented advancement. It must be a great satisfaction to us all that the action of Great Britain in encouraging coöperative research is being carefully studied in this country, and the advisability of organizing a similar system of coöperation here is actively discussed.

In view of this great world-wide movement in the broad question of industrial research, I have some hesitation in addressing you this evening in regard to the scientific research that has been conducted and is now under way in a relatively small field of industry. Yet it is of interest to note that at the time Mr. Little delivered the address to which I have referred the research laboratories of the National Canners' Association were being equipped and organized and, at the time the ideas of Great Britain on the subject of coöperative industrial research were formulating, these laboratories had been established and afforded the first instance in this country of a permanent organization for coöperative research maintained by an organized industry.

The canning industry has presented a field with which science has played small part. The first experimental work from which was evolved the process of sterilizing in hermetically sealed containers was conducted before the day of Pasteur and before the

idea of sterilization was conceived. The present canning technology was developed by means of an immense amount of experimental work, sometimes conducted in a systematic manner, but often more or less haphazard. The records of this experimental work are not available. Thus countless repetitions have occurred and a world of effort and experience has been lost. With trained observers, experienced in scientific research, much greater progress might have been expected.

From time to time various problems and difficulties were referred to scientific men not familiar with the industry, but, for the most part, the problems given them were incompletely or incorrectly stated or the workers had not the opportunity to grasp the full scope of the questions submitted to them.

Still there were several incursions of scientific men into the problems of the canning industry which resulted in great good. Among these may be especially mentioned the work of Russel in 1895, of Prescott and Underwood in 1896 and on three subsequent occasions, and of Harding and Nicholson in 1903. These articles—all on bacteriological subjects—while describing work which was not carried out to the extent that might have been desirable, were fundamental in their character and gave to the industry new data and new ideas whose value cannot be overestimated.

The idea of systematic research in connection with the canning industry is a very recent development. Only a relatively short time ago the industry was governed largely by trade secrets, and the methods of the canner were shrouded in mystery. Within the memory of men still operating the process, man was supposed to be possessed of secrets which were guarded from even the proprietor of the establishment in which he was employed, and the process-room was a place of mystery to which no one but the man who controlled its operations was admitted. At the present time this condition of affairs has been reversed, and technical processes are now openly discussed. The operatives of competing manufacturers commonly consult each other with the utmost freedom regarding the technology of the industry, and such consultations result in material progress. This transition from the era of trade secrets to the era of helpful collaboration was not a simple step, but rather a gradual development. The important thing is that it has been accomplished.

In connection with this transition, there has grown up the recognition of the need of systematic scientific study, which has been somewhat gradually developed and has been placed upon a coöperative basis. Unfortunately, this work has had to be extended over a greater field than would have been necessary if coöperative research had been general in the industries of the country. It has been necessary to take up as fundamental some of the questions which should have been studied earlier by other industries. For this reason the work has not been as intensive as it would otherwise have been.

The interests and responsibilities of the canner cover a much broader field than is usually supposed. They begin with the planting of the seed and continue until the finished product is finally consumed. The canner must give active personal attention to the growing of his crops, because some of his most important problems are purely agricultural. The acreage must be ample and the seeding so arranged that the harvest will be distributed with as much uniformity as practicable over a relatively long season, in order that the plant may be kept in operation regularly and for the maximum time. The greatest care must be exercised to harvest each field at the proper stage of maturity. The raw product must be of high grade, or the finished product will be inferior. Finally, the crop must be profitable to the grower, or he will turn his attention to other crops.

The canner, therefore, in addition to securing contracts for the acreage he desires, must give his personal attention to the character of seed planted, the methods of cultivation, the use of fertilizers, and the fighting of insect and fungous pests that may be encountered. Fortunately, all of these problems come within the general field of agriculture, and an army of well-trained men have given them attention in the Department of Agriculture and in the various State Experiment Stations. These agencies, moreover, are always ready to give assistance and to investigate new problems that may arise in their fields. Still, such new problems are constantly arising, and especially problems which are new to an individual locality, and the canning industry could not succeed without keeping in close touch with the various agencies engaged in agricultural research.

After the raw product is received at the canning plant, each of the operations necessary for the completion of the canned

article presents problems which often require investigation. An experienced employee must make frequent inspections of the product at every stage of its preparation and, whenever any abnormal condition appears, the cause must be discovered and removed.

The problems that present themselves are of the most diverse nature. Difficulties frequently arise while the product is being canned and must be solved quickly and remedied if a normal product is to be secured. More frequently, however, difficulties are encountered after the product is canned. They may be apparent when samples are opened for inspection as soon as the canning operations are completed, or they may develop on storage. Such difficulties can be adequately studied only by men who are thoroughly conversant with the technology of the industry.

Among the experienced men of the canning industry there is already a world of information regarding questions of this kind. Unfortunately, this information is not always accurate, and opinions of experienced packers are frequently conflicting. Many skilled operatives have considered only a part of the conditions under which they were working and, as a result, have drawn wrong conclusions. The attention to such questions of trained observers, therefore, must necessarily be of value.

As an illustration of the difficulties referred to may be mentioned a general dark-gray discoloration in canned corn. This has been encountered rarely, but where it has been found it occurred in considerable amount. Its study showed that it was due to a mere trace of copper coming from the cooker in which the corn was heated before being filled into the can. While the plant was in operation this color was not produced. It sometimes happens, however, that as a season nears the end a plant shuts down for a few days and then starts again to take care of some belated fields. In this interval a slight tarnish forms on the copper surface of the cooker valves and is not removed by careful scrubbing. When the canning of the corn is resumed this film of copper oxide is dissolved and passes into the corn. Then, when the corn is sterilized after the can is sealed, the infinitesimal trace of hydrogen sulphide which is always formed when any food is cooked converts the dissolved copper into sulphide, which, in the colloidal form, exerts a marked coloring action on the product. In this way one ten-thousandth of one per cent. imparts

to the corn a dark-gray color which is unattractive to the eye. The cause being known, the remedy was obvious, and this particular difficulty caused no further trouble. This one instance is cited merely as an illustration of a broad class of problems that arise from difficulties in the daily work of the industry.

SPOILAGE.

Spoilage in canned foods betrays itself either by the swelling of the can or by the abnormal taste or appearance of the food. Usually it occurs rapidly and is evident a day or two after the food is packed. It is always due to faulty technic, and it is of the utmost importance to the manufacturer to locate the cause of the spoilage and provide a remedy at the earliest possible moment.

When we remember that the rate of penetration of heat through the contents of a can depends largely on the physical nature of the food, it is evident that slight changes in the character of the food will greatly influence the time of heating necessary for sterilization.

The temperature and time required for the sterilization of a particular food sometimes vary in different seasons and in different parts of the country. It may happen, therefore, that a canner will find his product undergoing spoilage, though it was put up by a method which had always been satisfactory before.

Again, spoilage is often due to the can not being properly closed. The brief duration of the canning season makes it difficult to have the factory manned with a sufficient number of highly skilled operatives. It sometimes happens, therefore, especially when working with a new type of can, that the closing machine will not be properly adjusted, and cans closed with it will contain a leak so slight as to be very difficult of detection and yet sufficient to cause the contamination of the contents.

Then the process man may be misinformed regarding the capacity of some machine he is using. Sometimes the temperature is not uniform in all parts of the sterilizing equipment, so that in processing the cans do not receive the temperature indicated for the required time.

When we consider that a given case of spoilage may be due to one or several of these causes or to a variety of similar reasons, it is apparent that the cause of spoilage is frequently

difficult to determine, and both skill and experience are necessary to make this determination accurately and quickly.

But spoilage does not necessarily occur while the goods are being packed. It is sometimes due to organisms which multiply slowly, especially at the temperature at which the goods are stored. In such cases it appears only after the goods have been stored for a considerable time, either in the canning plant or after distribution. When spoilage of this nature is discovered it is, of course, important to locate its cause as promptly as possible and to determine its probable extent.

In connection with a question of spoilage, however, whether it occurs while the food is being packed or while it is in storage, the important thing is to understand and learn to safeguard against it.

BY-PRODUCTS.

Much has been said of the possibility of utilizing by-products of the canning industry. On the whole, great progress has been made in the matter. Many products, such as cobs and husks of corn and the vines of peas, are used directly for feeding stock or are preserved in silos for use in winter.

A relatively small number of by-products are used in the manufacture of fertilizers or are applied directly to the fertilization of the soil. There are still, however, countless tons of material whose removal is only an expense to the industry and for which no use has yet been found. It is frequently pointed out that much of this material has a certain food value and, if placed in such form that it could be preserved and transported, would add to the food supply available for our domestic animals. In these matters, however, it must always be kept in mind that to dry a product containing 90 per cent. or more of water is a relatively expensive operation. When we consider the large volume of this material in a single canning plant it will be understood that the drying of the offal would require an elaborate and expensive equipment. Most canning plants are operated only for a few weeks in the year and on this account cannot consider an expenditure for "overhead" which could be carried readily by a plant running continuously.

It must also be remembered that, during the canning season, all the facilities of the canner are taxed and it is a difficult matter for him to secure the labor necessary for his ordinary operations—

let alone the drying of the by-products and placing them in suitable form for shipment. It has been suggested that such by-products be prepared for commerce at central plants to which they would have to be delivered from the separate canneries. Even if this were done, however, preliminary drying would be necessary at the canning plant. In summer and early fall, when most of the canning is done, spoilage occurs rapidly, and all by-products, if they are to be handled at all, must be dried immediately.

This question is now being studied by competent engineers. I have no doubt much progress will still be made in the matter of disposing of the by-products of the canning industry. It must be borne in mind, however, that the question has limitations which are not apparent to the cursory observor, who is inclined to criticise the canner when he sees tomato peelings or pomace hauled away from the plant at a direct expense.

SANITATION.

In addition to the peelings, trimming stock, and other solid material just referred to, there is frequently a considerable amount of liquid waste and wash-water whose disposal is difficult in some localities. Under some circumstances such products become a nuisance in the community, and their cure offers new problems in sanitary engineering. This is a matter to which sufficient attention has not been given, but it is being studied by the Public Health Service and other agencies.

Methods of treatment have been devised for handling the liquid waste from some types of canning plants, and progress has been made in others.

TIN PLATE.

Industrial research in the canning industry must cover a much broader field than would be necessary if coöperative industrial research had been adopted by your manufacturing industries a generation ago. Defective tin plate has been believed by some canners to be responsible for many difficulties of the industry.

Under bad storage conditions cans frequently rust on the outside. This gives them an unattractive appearance and, if the rusting continues, sometimes causes perforation of the can and spoilage of the contents. With some light-colored products, such as corn, patches of a dark pulverulent substance occur on the

inside of the can. Sometimes acid fruits perforate the cans and thus permit the contents to spoil.

These phenomena and many others have long suggested the thought that imperfections in the tin plate or faults in its manufacture might be responsible for some of the canner's troubles. Many studies of tin plate were undertaken, but the expense involved was so great and the amount of detail so enormous that the investigations were not made sufficiently comprehensive to be of material value.

Finally, in 1915, a collaborative study was begun of the relative value of different weights of tin coating on canned food containers. This investigation was conducted by a joint committee representing the laboratories of the American Can Company, The American Sheet and Tin Plate Company, and the National Canners' Association. Two representatives of the Bureau of Chemistry were also associated with the committee, participating in all the work and taking part in the discussions. The committee reported early in 1917.

As might be expected in an investigation of this scope, the study suggested more question than it settled. Yet it did afford conclusive answers to some important questions. Among these are the following:

1. None of the difficulties with canned foods that have been attributed to tin plate are caused entirely or even largely by light or defective coating.

2. No weight of coating will overcome faulty technic or faulty storage in the finished product.

3. When cans are stored under conditions that promote rusting, heavy coating will retard rusting, but will not prevent it.

4. The various discolorations on the inner surface of the can and in the contents have no relation to weight of coating.

But it is evident that there may be other considerations in tin plate than weight of coating. The importance of the composition of steel and of the conditions of its treatment in other industries emphasize the probable importance of the same considerations in connection with tin plate. As stated above, the canner is interested, among other things, in the rusting of the exterior of the can, the corrosion and formation of undesirable colors on the inner surface, and in localized corrosion resulting in perforations and leading eventually to spoilage. We cannot

tell, without a comprehensive experimental study, whether these conditions will be influenced by the composition of the steel, the character of its surface, the temperature of annealing, or other conditions of manufacture.

A comprehensive collaborative investigation of this subject was instituted a few months ago by the laboratories of the American Can Company, the Titanium Alloy Manufacturing Company, and the National Canners' Association. This investigation may require several years for its completion, but it is being conducted on so elaborate a scale that it is believed the results will be conclusive and fundamental.

FOOD POISONING.

The responsibility of the food manufacturer does not cease even when the food has passed into the hands of the consumer. There is a general popular impression that food poisoning is likely to be produced by this food or that, and many people are prone to attribute an attack of indigestion to poisoning caused by a particular food.

Then it is well known that cases of food poisoning do occur, due to the infection of the food at some period in its history. This infection may occur at the time of the original harvesting of the food or at the time of its preparation in the form in which it is placed on the market or it may occur in the preparation of the food for the table after it reaches the hands of the consumer.

In any case, our information regarding the general question of food poisoning is altogether inadequate, and it is of the utmost importance to manufacturers and handlers of food that our information on this subject be made more complete. This is also of importance to the medical profession, and especially to the public.

The canning industry has long felt the need of an adequate survey of this subject by responsible parties, and something more than a year ago invited the National Research Council to organize a systematic investigation of this subject and designate people who were to conduct it, the investigation being supported by the industry. After careful consideration, the National Research Council accepted this invitation and organized the investigation, naming as director, Dr. M. J. Rosenau, head of the Department of Preventive Medicine and Hygiene of Harvard Medical School. The work was undertaken in June and is being actively continued.

THE PRESENT CONDITION OF THE INDUSTRY.

In view of the unprecedented conditions that now confront us, it seems best to digress for a few moments from the subject of the evening and discuss briefly the present situation in the canning industry.

During the last season there were packed, in round numbers:

Tomatoes	15,000,000 cases, of 24 cans each.
Corn	11,000,000 cases, of 24 cans each.
Peas	10,000,000 cases, of 24 cans each.
Other vegetables	13,000,000 cases, of 24 cans each.
Fruits	11,000,000 cases, of 24 cans each.
Evaporated and condensed milk	30,000,000 cases, of 48 cans each.

When we add to this total of 90,000,000 cases the pack of other varieties of canned foods, including the various kinds of fish and shellfish, and an unprecedented pack of meat products, we have a grand total of more than six billion cans.

In normal times a pack of this size would mean low prices, yet high prices prevail and must prevail, as they do in other industries. Everything the canner uses has increased in cost: cans, labels, boxes, nails, sugar, coal, machinery—every article used directly or indirectly in the operation of the factory and in preparing and shipping the food. Because of the shortness of the canning season an adequate supply of skilled labor is scarcely available even in normal times. Now the difficulty of securing labor presents an obstacle that is almost insurmountable.

Then we have an enormous increase in the cost of raw products. The grower gives preference to the crop that will give him the greatest return. It is no simple matter to pay enough for canners' crops to compete with $2.25 wheat and with corn at a proportionate price. Moreover, wheat and corn require little labor. Most canners' crops require much, and labor is not available. Perhaps the cost of producing canned food under present conditions is best understood when we remember that both canners and wholesalers are licensed by the Food Administration and are not permitted to make a higher charge for staple products than is necessary to yield them a fair profit.

No prediction can yet be made regarding the probable pack of the present year. The Army and Navy will require about 35 per cent. of a normal pack of tomatoes and 25 per cent. of a normal pack of corn, peas, salmon, and sardines. The Army and Navy and the civilian population of our Allies will require ten

million cases of milk if bottoms can be secured for its transportation. All these products must go from America, for no country in Europe can grow the food needed by its own people. It is known to all that canned foods are preserved by heat alone in hermetically sealed containers. They afford us the nearest approach we know to fresh cooked food. It is only in this form that the substantial equivalent of fresh foods can be supplied to our Army and Navy and to the military and civilian population of our Allies. Almost every variety of food that is cooked can be preserved by canning and is actually on the market in the can.

Desiccated foods have a promising outlook, but with many products are still in the experimental stage. Desiccated tomatoes, corn, and peas are unknown in a commercial way, except for the preparation of soups. We do not know whether they will be acceptable to the public. We know little of the technological difficulties still to be overcome in their commercial production. So far as experience has gone, there is no reason to believe that the manufacture of desiccated foods may not grow to large proportions—but it must be a growth. A large industry cannot spring into being. In any case, the present wants of the Army and Navy for tomatoes, corn, and peas must be supplied with the canned product.

No estimate can be made of the extent to which growers will be willing to contract for acreage in canners' crops.

Supplies of pig tin are always a source of anxiety. The amount on hand is never sufficient to make the future secure. Recently the condition in this respect has been less satisfactory than usual. Our chief anxiety regarding the supply of tin plate, however, is the lack of production of both plate and cans owing to transportation difficulties.

The plans of the early winter for the manufacture of plate and cans were impossible of execution. The many demands on the transportation system made it impossible to move plate from its place of production to the makers of cans, and, as a result, some can-making plants found it necessary to shut down for a time because of lack of plate, while, on the other hand, some plate manufacturers filled their storerooms and then were compelled to close their plants because of lack of additional storage space. Under these circumstances, accurate predictions cannot be made. We can only say that the requisite number of food containers for the coming season will not be available.

A METHOD FOR THE DETERMINATION OF THE RELATION BETWEEN VAPOR PRESSURES AND THEIR CORRESPONDING TEMPERATURES AT PRESSURES OF LESS THAN FIVE MILLIMETRES.*

BY

W. R. HAM, Ph.D., J. C. CHURCHILL, M.S., and H. M. RYDER, M.S.

THE object of this work was the development of a method whereby the relations between vapor pressures and their corresponding temperatures might be determined for pressures of less than five millimetres, and especially for pressures of less than one millimetre of mercury.

Most data published to date on the matter of vapor pressures are confined to minimum pressures of five millimetres of mercury or greater, although a few give values of one millimetre. This has been partly due, no doubt, to the difficulties in the past of obtaining a fairly high vacuum in a large system, or in a system which could not be hermetically sealed, and partly to the inability to read low pressures accurately by means of an ordinary mercury column. The most accurate readings obtainable with any ordinary mercury column, or with a U-tube, will probably vary at least a few tenths of a millimetre from the true value. Ramsay [1] did not rely on a U-tube manometer for values of less than 2.5 millimetres. The McLeod gauge has been known for a long time, but results obtained from it are of no value where the gas contains vapor, or even for such gases as air, nitrogen, etc., which probably condense to a certain extent. Ramsay [2] found the McLeod gauge to be reliable for hydrogen down to a very low pressure by comparing gauges of different dimensions, and by calculating the values, knowing the volume of the cylinder of his pump and the volume of his system. A method which would permit of measurements of vapor pressures by a McLeod gauge, and at the same time would permit of the use of pure hydrogen in the gauge, would enable low vapor pressures to be measured

* Communicated by Doctor Ham.

[1] Travers, "Experimental Study of Gases."

[2] *Ibid.*

accurately. In recent years vacuum pumps have been placed on the market which will pump very rapidly down to 0.05 millimetre on a small closed system, and to a somewhat higher pressure on large systems, thus making practicable the consideration of the determination of very low vapor pressures.

Biot, according to Preston,[2] expresses the relation between

the vapor pressure of a substance and its corresponding temperature in the formula

$$\log P = A + Bb^T$$

or more generally

$$\log P = A + Bb^T + Cc^T + Dd^T + \ldots\ldots,$$

where A, B, C, D, b, c, d are constants to be determined by experiment.

Roche[3] offers

$$P = Aa^{m^T} - n^T$$

but which Regnault[3] found to be somewhat more approximate than that offered by Biot.

Clausius[4] expresses the relation between vapor pressure and temperature by means of the formula

$$\frac{d \log P}{dT} = \frac{-L_2}{RT}$$

[2] Preston, "Theory of Heat."

[4] Knox, "Physics—Chemical Calculation," p. 25, 1912.

where P is the pressure, T is the absolute temperature, L is the molecular heat of evaporation, and R is a constant, its value depending on the units used. If L were a constant this would give an equation easily handled where L is known, but as L is a variable, and in general some function of T, the equation becomes involved, if not impracticable. A combination of this equation and Biot's shows that

$$L=RT^2b^T$$

which is not a very convenient quantity with which to work.

Rankine [5] offers the formula

$$\log P=A-\frac{B}{T}-\frac{C}{T^2}$$

with the values as above. In offering this formula Rankine presents the first two terms of the right-hand side of the equation from a theoretical consideration of molecular activity. These considerations, however, leave undetermined a variable quantity, which is, in general, some function of the density of the liquid, which in turn is variable, and the change in what he pleases to call the "Superficial atomic elasticity" of the liquid or solid from which the vapor is obtained. To correct this, he adds arbitrarily

[5] Rankine's "Miscellaneous Scientific Papers," Griffin, 1881.

the third term, $\frac{C}{T^2}$, which form suggested itself from a study of the variations between observed results and those calculated from the equation, not using the third term. Rankine's formula and the one offered by Biot are used in later considerations.

In connection with this work, the term "Boiling" of the liquid may hardly be said to apply, as at the lower pressures the giving off of vapors by the liquid is not accompanied, necessarily, by ebullition. This is probably due to the fact that the pressure

on the liquid a few millimeters below the surface is very much greater, relatively, than at the surface, and a considerably higher temperature would be required to cause a rapid formation of vapor. The purity of the liquid and the smooth interior of the vessel would probably also permit of considerable superheating of the liquid before ebullition would take place.

The accompanying photographs and diagram will give an idea of the apparatus used. The still is shown on the right, in the diagram, and consisted of a chamber, *a*, for heating the liquid; a chamber, *b*, in which the thermometer was placed, and which was covered with tin foil and cotton during the tests; a condenser, *c*, with a water jacket; a receiver, *d*, which collected the distillate;

a second receiver, *e*, which was packed in ice and which served to keep any vapor from passing to the rest of the system; an electric heating element, *f*, consisting of a platinum coil wound on a glass rod and immersed in the liquid, which was the source of heat, and which was controlled by means of the rheostat, *g*. The leads from the exterior to the heating element consisted of glass tubes through a rubber stopper, a mercury column serving as conductor in each.

The remainder of the apparatus consisted of the remaining portion of the vapor system, and the hydrogen system. A drying

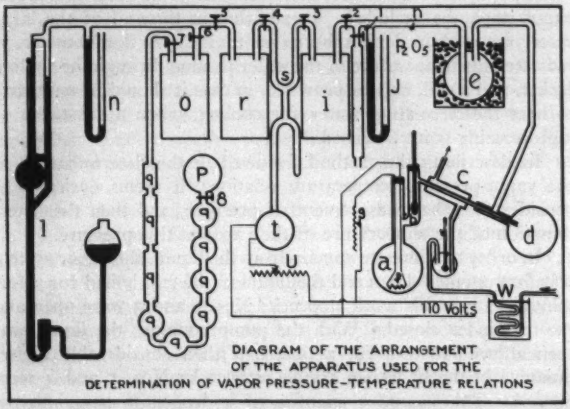


DIAGRAM OF THE ARRANGEMENT OF THE APPARATUS USED FOR THE DETERMINATION OF VAPOR PRESSURE-TEMPERATURE RELATIONS

tube, *h* (phosphorus pentoxide), was placed next to the cold receiver as a further provision against the movement of any vapor to the remainder of the system. The reservoir, *i*, of about 20 litres capacity, was used to prevent any sudden fluctuations in pressure and to render negligible any small leaks. A U-tube, *j*, was used as a rough indicator of the pressure in the system.

The hydrogen system consisted of a McLeod gauge, *l*, with a cathetometer; a U-tube, *n*, a hydrogen reservoir, *o*, which was supplied with hydrogen from a Kipp generator, *p*, the hydrogen being washed and dried by passing through the bottles and tubes, *q*; and a reservoir, *r*, used, as in the case of the vapor system, to render the pressure in the system more stable.

The two systems were joined at *s*, with the tubing shaped as shown, and connections were also made here with the pump. Provision was here made for a second pump, in case more rapid pumping should be desired. The pump used was a May-Nelson two-ring pump, *t*. The electrical circuit was arranged with a rheostat, *u*, so that the speed of the pump could be readily controlled.

In order to seal the system as perfectly as possible, all stoppers and rubber-tube connections were sealed with mercury, and all stopcocks were similarly protected. Whenever air was let into the vapor system it was first thoroughly dried in order that water vapor might be excluded. In addition to this, all of the large reservoirs contained phosphorus pentoxide. A thermometer, *v*, indicated the temperature of the water passing through the cooling jacket, and a coil, *w*, was provided, in case it should be necessary to heat the circulating water for cooling, when a substance of high freezing-point was used.

In describing the method followed in the determination of the vapor-pressure temperature relations, it seems desirable to consider first the measurement of pressure, and then the determination of the temperature of the vapor at this pressure.

In order to eliminate any air from the liquid, the vapor system was first pumped down and the liquid in the still boiled for a few minutes. For this work stopcocks Nos. 2 and 3 were open and the remainder closed. With the pump running, the liquid was then allowed to cool for a time. With a considerably higher pressure in the hydrogen system, stopcocks Nos. 5 and 4 were opened. This caused a quantity of hydrogen to pass into the vapor system, and the pump then worked on both the hydrogen and the vapor systems. The pump was permitted to continue for several hours, the degree of vacuum at the end of this time being determined by the speed of the pump. If a fairly high vacuum was wanted, the pump was operated at a higher speed.

When the desired vacuum was reached, the reservoir of the McLeod gauge was raised and stopcock No. 3 was closed, and the determination of the temperature was carried on in the closed system. The pump was, however, permitted to operate on the hydrogen system, in order that the pressure on the mercury column in the McLeod gauge might be reduced to a negligible quantity, if it were not already so. The gauge was permitted to

stand for some time, and the readings were then taken and the pressure calculated. The pressure thus obtained was assumed to be the pressure in the receiver of the still. This assumption was allowable for the following reasons:

If there was no flow of gas in the system, the pressure in the gauge would be the pressure in the system at the receiver. To approach this, the pump was operated very slowly toward the last, so that the flow of gas was reduced to a minimum. Further, the pump was working on the apparatus at almost its exact centre, so that what flow of gas there might be, was from either end toward the centre. Theoretically, if the two halves of the system offered equal resistance to the flow of gas, the pressure in the receiver would equal the pressure in the guage. However, even though this was not exactly the case, the effect of the flow on one side largely counteracted the effect of the flow on the other side, and this fact, together with the fact that the flow was reduced to an almost, if not altogether negligible quantity, will show that the assumption made above does not detract from the value of the results.

Other theoretical approximations might result from leakage in the system and from inaccuracies in the working and reading of the gauge itself. The system was carefully tested as to its ability to hold a vacuum, and the leaks were found to be negligible. From tests made on the gauge, it was found that if the gas was highly compressed in the capillary tube the readings were somewhat lower than when it was not so highly compressed. However, with the longer column of gas above the mercury in the closed tube, for various lengths of column, very consistent readings were obtained. It was also observed that on expanding and contracting the gas the readings as calculated from the gauge would not change. Any vapor in the gas would cause very erratic readings, but it is very improbable that vapor would get into the gauge in this system, since it was originally filled with pure hydrogen, and after pumping began the flow was always away from the gauge and through a system of considerable capacity.

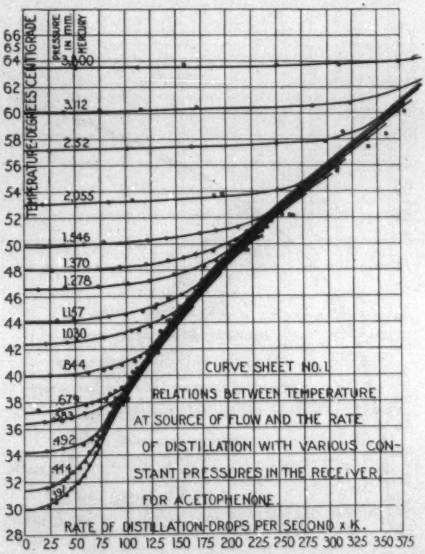
In order to determine the temperature of the vapor at the pressure in the receiver, it was necessary to make the pressure at the thermometer equal to the pressure in the receiver and at the same time have a saturated vapor enveloping the thermometer. This is easily accomplished at relatively high pressures, but at

very low pressures a very slight flow of vapor from the thermometer chamber to the receiver might mean an enormous relative increase in the pressure at the thermometer over the pressure in the receiver. On the other hand, if the flow of vapor ceases, it is very probable that the thermometer is not entirely enveloped in a saturated vapor, and hence it would indicate values too low. In view of this, in order to accurately determine the temperature of the vapor corresponding to the pressure in the receiver, the following procedure was adopted:

The liquid in the still was raised to such a temperature that there was a considerable flow of gas past the thermometer, as evidenced by the condensed vapors flowing into the receiver from the condenser. The pressure at the thermometer was then obviously greater than in the receiver, so that the thermometer reading must be greater than the desired value. Observations were then taken of the rate of distillation (a measure of the current of vapor) as evidenced by the number of drops of liquid entering the receiver in a given time, and the thermometer reading. The rate of distillation was then reduced and readings were again taken. This process was repeated until the movement of vapor almost ceased. A curve was then plotted (see curve sheet No. 1) between pressure as ordinates and the rate of distillation as abscissæ, and this curve extended to the point of zero rate of flow. The temperature corresponding to this condition must necessarily be the temperature of the vapor at the thermometer at the condition of zero rate of distillation; that is, with the pressure at the thermometer the same as the pressure in the receiver. Knowledge of the pressure in the receiver will then furnish the necessary data for one point on the vapor pressure—temperature curve for the liquid. A repetition of this process for other pressures in the receiver will enable any number of points on the curve to be determined. Curve sheet No. 1 shows a series of curves plotted between temperature readings of the thermometer and rates of distillation for various pressures in the receiver. It will be noticed that each curve, to a certain point, follows a general one (moving from right to left), which is convex upward. From this point the curve becomes convex downward, and approaches a horizontal line whose value in degrees, as has been stated before, is the temperature corresponding to the vapor pressure in the receiver, since at this point the pressure in the

receiver is the same as at the thermometer. At all other points on the curve, then, the pressure at the thermometer is greater than in the receiver.

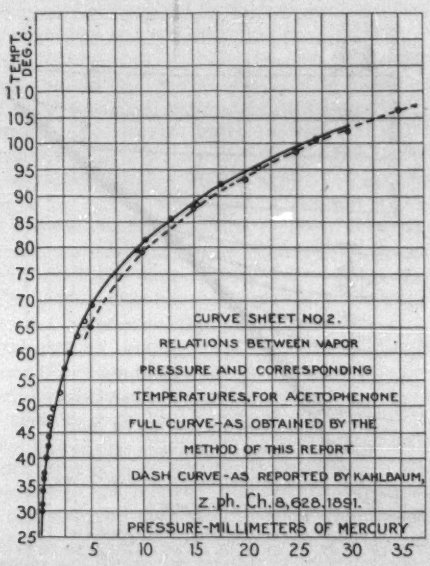
Since the rate of distillation is given in terms of drops of condensed vapor passing into the receiver in a given time, and since, when being formed rather slowly, the drops of a given liquid will, at a given temperature, be sensibly the same in size,


TEMPERATURE-DEGREES CENTIGRADE
PRESSURE IN MM. MERCURY
3.800
3.112
2.52
2.055
1.546
1.370
1.278
1.157
1.030
.844
.679
.583
.492
.414
.391
CURVE SHEET NO. 1
RELATIONS BETWEEN TEMPERATURE AT SOURCE OF FLOW AND THE RATE OF DISTILLATION WITH VARIOUS CONSTANT PRESSURES IN THE RECEIVER, FOR ACETOPHENONE.
RATE OF DISTILLATION-DROPS PER SECOND x K.
0 25 50 75 100 125 150 175 200 225 250 275 300 325 350 375
66 65 64 62 60 58 56 54 52 50 48 46 44 42 40 38 36 34 32 30 28

and therefore weight, and since the temperature of the condensed vapor was at all times practically constant, the measure of the rate of distillation as made will also be a measure of the current of vapor passing the thermometer, in terms of some constant times the weight of the vapor passing a point per unit time.

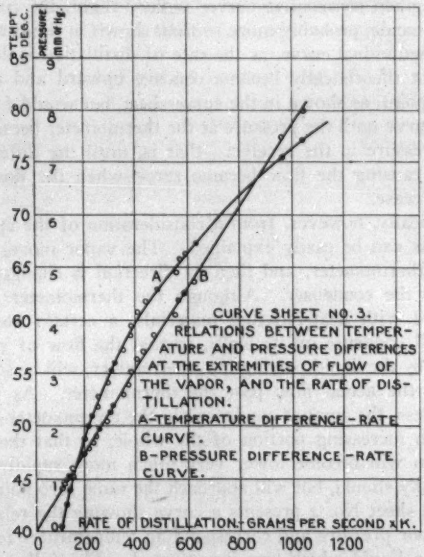
The general curve referred to is, then, a curve plotted between the current of vapor and the temperature as recorded by the thermometer. Since there is some relation between vapor pressure and temperature ($T = f(P)$), and since the cause of the flow

is $(P-P_o)$ where P is the pressure at the thermometer and P_o is the constant pressure in the receiver, the curve gives a relation between the same function of the difference of pressure, with the zero point of the ordinate properly transformed, and the current. If a curve is plotted between the current and P, instead of between the current and $f(P)$, this curve expresses the relation between the difference of pressure and the current. The slope of this



curve at any point will, then, be a measure of the resistance the path offers to the flow of the current under the particular conditions. Curve sheet No. 3 shows curve A plotted between the temperature readings of the thermometer and the current. Curve B is a similar curve showing the relations between the current and P, instead of $f(P)$, the values of P corresponding to t being obtained from curve sheet No. 2. It is obvious that curve B is a straight line passing through the origin. This would indicate that the quantity of matter passing from the thermometer

chamber to the receiver per unit time is proportional to the difference of pressure. That this should be so, within the limits of accuracy of the method, will be obvious, since the physical dimensions are constant for the path, since the linear velocity of the flow is inversely proportional to the density of the vapor, and since any increase in friction or viscosity due to an increase in density is a quantity too small to realize in a work of this sort.



This straight-line curve will also serve as an excellent check on both the temperature-rate curves and the vapor pressure-temperature curve, as each of these curves contains quantities which are independent of the other.

In the actual plotting of the curves on curve sheet No. 1, the points on the upper parts of the curves were not determined with sufficient accuracy to show that the curves become parallel, as they have been drawn, rather than coinciding. This must be the case, however, for, considering any two curves, at any particular temperature reading, the pressures for these curves in the

receiver are different, while at the thermometer they are the same. The difference of the pressure causing the flow, for any two given curves for any constant temperature, will cause different currents of vapor to flow. That is, the rate of distillation will be different for any two curves for any given temperature reading of the thermometer. Hence the curves cannot coincide. However, since the difference in the pressures in the receiver for any two given consecutive curves is very slight, the curves will nearly coincide, probably more so than shown in the curve sheet.

Any individual curve, as the rate of distillation is decreased, would not theoretically become concave upward and approach the zero point, as shown in the curve sheet, but would follow the general curve until the pressure at the thermometer became equal to the pressure in the receiver—that is, until the difference of pressure causing the flow became zero—when the flow would suddenly cease.

Practically, however, from a consideration of the apparatus, the results can be easily explained. The vapor moves upward past the thermometer, and then its direction is changed and it passes to the condenser. Although the thermometer chamber is covered with heat-resisting materials, a certain amount of vapor will condense on its walls, so that the flow of vapor, as recorded by the liquid passing into the receiver, will be somewhat less than the actual flow past the thermometer. As the flow becomes less, the amount condensed in the thermometer chamber will be an increasing portion of the whole, so that the rate of distillation will become lower very much more rapidly than it theoretically should, but will approach the same zero point.

Curve sheet No. 2 presents a curve showing the relation between vapor pressure and corresponding temperatures for acetophenone, a liquid which was investigated. The full curve was obtained by plotting the receiver pressures as measured against the corresponding lower limiting temperatures as obtained from curve sheet No. 1. The broken line curve was plotted from Kahlbaum.[6]

The extreme lower end of Kahlbaum's curve does not coincide with the one here obtained, but they rapidly come together as the pressures increase. According to the reference given above, Kahlbaum measured his pressures by comparing the height of a mercury column connected with his system with the baro-

[6] *Zeitschrift für Physikalische Chemie*, 26, 603, 1898.

metric height, which method cannot be depended on for accurate results at low pressures.

The equation suggested by Biot as a general one for this curve, and which has already been mentioned—that is,

$$\log P = A + Bb^T$$

has been applied to the results here obtained, in combination with Kahlbaum's results. Points on the curve corresponding to pressures of 4, 300, and 760 mm. were used for the determination of the constants. The particular form of the equation as obtained was

$$\log P = 19.696 - 72.540 \times 0.993944^T$$

Vapor Pressure—Temperature Relations for Acetophenone.

TABLE I.

Temperature degree C.	Observed P, mm.	Kahlbaum's P, mm.
30	0.391	
31.65	0.444	
34.25	0.492	
36.45	0.583	
37.2	0.679	
39.8	0.844	
42.4	1.030	
44.1	1.157	
46.55	1.278	
48.0	1.370	
49.8	1.546	
54.0	2.055	
57.15	2.520	
60.05	3.112	
63.50	3.800	
65.0		5.0
66.0	4.50	
69.2	5.20	
79.0		10.0
79.7	9.6	
81.2	10.2	
85.4	12.8	
87.4		15.0
87.7	15.4	
92.0	17.7	
93.3		20.0
95.4	21.4	
98.2		25.0
100.3	26.8	
102.3		30.0
106.0		35.0

Values of P were found for corresponding values of T from this equation for various temperatures, and the results are shown in Table II, together with the values of P as obtained from the curve. It will be seen that this equation is an approximation, and not by any means accurate, although the points most liable to show some difference between the calculated and observed values were the ones chosen.

TABLE II.

Temperature, degrees C.	Observed P, mm.	Calculated P (Biot), mm.	Calculated P (Rankine), mm.
30	0.39	0.44	0.45
50	1.60	1.65	1.705
106	35.0	31.0	32.6
145.1	150.0	146.0	146.3
177.5	400.0	401.0	400.6

"Observed" values of P over 26 mm. of mercury are those given by Kahlbaum.

The equation of Rankine,

$$\log P = A - \frac{B}{T} - \frac{C}{T^2}$$

was also applied to the present case, exactly as was Biot's. With the constants determined, the equation took the form

$$\log P = 16.152 - \frac{3390.96}{T} - \frac{534,192.38}{T^2}$$

Several values of P as found from this equation are found in column 4 of the accompanying Table II. It will be seen that the curve of one equation follows very closely that of the other, and it would be hardly proper to say that either has any advantages over the other, so far as accuracy is concerned, at least in the present case. They are probably equally cumbersome in their manipulation.

It would seem in general, then, from the foregoing, that the actual mathematical representation of the relation between vapor pressures and temperatures, either by means of equations based on theory or by purely empirical forms, has not as yet been satisfactorily accomplished. This relationship appears to be dependent on some phenomenon of which we have, as yet, slight knowledge.

December 12, 1917.

SIR WILLIAM RAMSAY.*

BY

RICHARD B. MOORE,

U. S. Bureau of Mines.

WILLIAM RAMSAY was born in Glasgow, Scotland, October 2, 1852, and died at his country home near London, July 22, 1916. In him the world has lost one of its greatest experimental scientists. A pioneer in most of his work, the results he obtained will undoubtedly be even more appreciated by future generations than by his contemporaries. Whether one accepts all of Ramsay's conclusions or not, there is no one who does not admire the man for his courage and skill, or his work for the new vistas it has opened up and the inspiration it has given.

Ramsay came from scientific stock. His grandfather on his father's side was originally a dyer, but later gave up this business to engage in the manufacture of heavy chemicals used in the dye industry. His uncle, Sir Andrew Crombie Ramsay, was professor of geology in University College, London, and for several years head of the British Geological Survey; while several of his relatives on his mother's side were physicians. Ramsay himself early showed a leaning toward science, especially chemistry. Even before he entered the University of Glasgow he "dabbled" in the science, using small quantities of chemicals furnished by his father.

Ramsay received his early education in the Glasgow Academy, and in 1866 entered the University of Glasgow, at the age of 14. Outside of his early matriculation, he seems to have made no special record at college. In 1869 he received his first real introduction to chemistry when he started work in the laboratory of Robert Tatlock. Later he became Tatlock's assistant, and worked at the same time in physics under William Thomson, who afterward became Lord Kelvin.

Ramsay's ambition was not satisfied with the chemical training he could obtain in Scotland. In 1870 he went to Germany, where, at first, he studied with Bunsen in Heidelberg; but in 1871

* Communicated by the Author.

he changed to the University of Tübingen, where he worked under Fittig and obtained his Ph.D. degree in 1873. The work he did at this time was on orthotoluic acid and the platinum ammonium compounds. The young chemist returned to Glasgow as assistant in the Young Laboratory of Technical Chemistry, and in 1874 received an appointment as assistant to Prof. John Ferguson, of the University of Glasgow.

He was not content to teach only, but at once started on the investigational work that brought him so much fame. The beginning was modest. His first paper, in 1874, was on a new antimony phosphide with a formula SbP. This was followed in the same year by a short paper on the properties and methods of preparation of hydrogen persulphide. In 1875 he published one paper. This was on the preparation of sodium ethylthiosulphate, and the reaction that takes place when this compound is heated with phosphoric chloride. In the following year he also published one paper—an analysis of a bismuth mineral carrying cobalt and nickel; and in 1877 produced his first paper in physical chemistry on "The Dehydration of Hydrates by the Time Method." Up to this time Ramsay's work showed no promise of what was to come. The problems were simple and were handled in a manner that showed no special ability. In the next few years, however, the young chemist seemed to find himself, and the work which he then did at least laid the foundation for the brilliant discoveries he made later on.

In 1878 and 1879 he published two papers in conjunction with J. J. Dobbie on "The Decomposition Products of Quinine and the Allied Alkaloids." The authors made the important discovery that quinine, quinidine, cinchonine, and cinchonidine, on oxidation, all gave the same acid, and for the first time showed the connection between the alkaloids and pyridine. The work of Kopp on specific volumes had interested Ramsay, and in 1879 he published a paper on "The Specific Volumes of Liquids at Their Boiling-points," which he termed their "ebullition volumes." It occurred to him, during the progress of the work, that some connection might exist between the "ebullition volume" of a liquid and the amount of heat evolved during its formation. In order to ascertain whether such a relation existed, he worked on the heat of formation of aniline, picoline, toluidine, and several other organic compounds, publishing the results in 1879.

In the following year Ramsay received the appointment of professor of chemistry in University College, Bristol, made vacant by the resignation of Doctor Letts. After one year of service he was promoted to the principalship of the college. At the time he was barely 30 years old; the college was still in its infancy, without proper support from the public, whom it was supposed to serve, and had already gone through several strenuous years. Ramsay remained at Bristol until 1887, and not only guided the institution through its many difficulties, but was able, at the same time, to publish a number of important papers. Dr. Sydney Young, his assistant, gave him invaluable service, and several of the papers were published under their joint names. The subjects treated were varied in character, but nearly all were of a physico-chemical nature. The volumes of sodium and phosphorus at their boiling-points were determined; the molecular volumes of some compounds of the benzene, naphthalene, anthracene, and phenanthrene series, and the atomic volume of nitrogen were studied.

Two papers published in the *Proceedings of the Royal Society* on "The Critical State of Gases" and "The Critical Point" led the author to make some very definite statements concerning gases and liquids, and to define the critical point as "that point at which the liquid, owing to expansion, and the gas, owing to compression, acquire the same specific gravity, and consequently are capable of mixing with one another."

In 1884 he published a paper on the halogen compounds of selenium, the work being undertaken in order to compare the stability of the halogen compounds of selenium with those of sulphur by measuring their amount of disassociation, caused by rise of temperature. With Doctor Young, he then studied the decomposition of ammonia by heat, paying special attention to the temperature at which decomposition took place and to the influence of the material of the vessel or tube containing the gas. The author's method of determining the temperatures was crude and novel. It was done by the fusion of salts placed upon small pieces of platinum foil lying on the surface of the tube. If calcium chloride and potassium chloride were used, and if the former salt liquefied, while the latter remained solid, the temperature was taken to be between 719° C. and 734° C. The same two authors showed, in a paper on "The Influence of Pressure

on the Temperature of Volatilization of Solids," that solids have definite temperatures of evaporation, corresponding to definite pressures, as liquids have definite boiling-points. "A Method for Obtaining Constant Temperatures" involved using convenient liquids, which could be obtained in a pure condition, and altering the boiling-points of such liquids by changing the pressure to which they were exposed.

These papers were followed by the publication of a series of vapor pressures, including those of mercury, bromine, iodine, and iodine monochloride, and a controversy with G. W. F. Kahlbaum as to whether Regnault's rule that "with pure substances the curves which show the relation existing between the temperature and pressure of a saturated vapor are identical, whether the method employed in obtaining the data used in their projection be the dynamical or the statical," was or was not true.

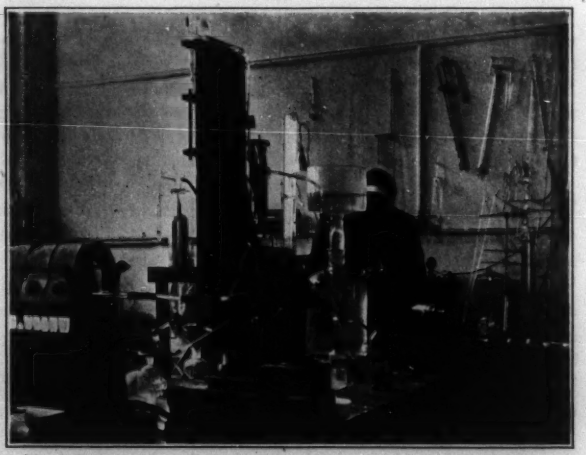
During 1886 and 1887 Ramsay and Young published a very considerable number of papers, the majority of them dealing with some phase of work in connection with vapor pressure. In the latter year Ramsay was chosen to succeed Professor Williamson as professor of chemistry at University College, London, and he remained there during the rest of his active professorial life. During the first few years at University College Ramsay worked along the same lines that had won him success at Bristol. It was during this period and the few preceding years that he acquired the training that made his subsequent work on the rare gases possible. He had not only discovered the value of skilful manipulation, but had developed a resourcefulness that stood him in good stead in his later and more important work.

Ramsay very soon got into a new field of chemical research which ultimately changed his reputation from that of a national one to an international one. The work was started through a partly accidental discovery. In 1882, and several succeeding years, Lord Rayleigh worked on the relative density of hydrogen and oxygen. In 1893 he published his results on the densities of some other gases, notably those of oxygen, nitrogen, and air. He prepared the oxygen and nitrogen by different methods; and, while the density of oxygen proved in all cases to be the same, that of nitrogen prepared by chemical methods differed from that of nitrogen obtained from the air. The latter was about 0.5 per cent. heavier than the former. A difference of this magnitude

could not be accounted for on the basis of experimental error, and the riddle remained unanswered for some time.

Finally Ramsay asked permission of Lord Rayleigh to take up the matter from a chemical standpoint. He had already in some previous work found that red-hot magnesium was a good absorbent for nitrogen. Hence he concluded that, if there was any part of atmospheric nitrogen different from the rest, a separation might be made by passing the gas over the heated metal.

FIG. 1.



View in Ramsay's laboratory across the centre of the room. On the centre table is shown part of the equipment for removing oxygen and nitrogen from a mixture of rare gases. The glass gas-holder contains partially purified argon, krypton, and xenon.

This was tried, the oxygen first being removed by metallic copper. A slight increase in the density of the product was observed. Arrangements were now made to circulate the gas backward and forward over the heated magnesium. After several days of this treatment the density was found to be 16.1. At this stage Ramsay did not know that he had to deal with a new element, but thought that the results were due to a triatomic form of nitrogen corresponding to ozone. A further absorption by magnesium finally gave a gas with density about 20. It was only when

the spectrum of this product was examined that Ramsay became convinced that he had found a new element. Meanwhile Lord Rayleigh was dealing with the problem in a different manner, passing sparks through a mixture of atmospheric nitrogen and oxygen in the presence of sodium hydroxide. The hydroxide absorbed the oxides of nitrogen formed, and, after removal of the excess of oxygen, the resulting gas showed an increase in density. From this point Ramsay and Rayleigh continued the work together, and announced the discovery of the new element at the British Association Meeting in 1894.

It was only natural that such an announcement should meet with a considerable amount of scepticism. All kinds of explanations were offered for the increased density of the gas obtained. It was only after further work had been carried out and the spectrum of the new element definitely established that the doubters were convinced. Since that time Ramsay's share in the discovery has been at times somewhat belittled.

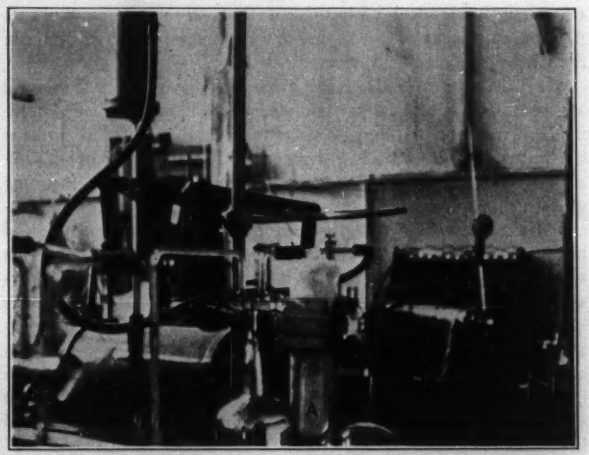
In 1908 the writer was at a dinner in London given to Sir William Ramsay. Lord Rayleigh responded to the principal toast on that occasion, and in his speech most emphatically gave the main share of the credit for the discovery of argon to Ramsay. While this was only just, it goes to show the good feeling which existed between these two notable scientists, and refutes the attempts that were made on several occasions to raise an issue between them.

Ramsay improved on his first method for making argon by using a mixture of lime and magnesium instead of the latter metal alone. The absorption of nitrogen by magnesium is a very slow process, but when mixed with lime the absorption is rapid and complete. The air is first passed over soda-lime and phosphorus pentoxide and then through a tube containing metallic copper, in which the oxygen is absorbed. From this it goes through the magnesium-lime mixture, over heated copper oxide, and finally through other soda-lime and pentoxide tubes. The amount of labor necessary to make argon in quantity by this method is considerable. The copper absorption tube finally used was an iron tube about 6 feet long and nearly 3 inches in diameter. The exhaustion of an apparatus on this scale and reduction of the copper oxide to copper again all involved a great deal of work:

but the final result was 15 litres of argon, which made possible the discovery of some of the other rare gases.

After the discovery of argon Ramsay looked for other sources of the element. He heard, through Sir Henry Miers, of a paper by W. F. Hillebrand, of the United States Geological Survey, in which it was stated that the mineral cleveite, on heating, gave off a considerable quantity of inactive gas, which was supposed to be nitrogen. Ramsay suspected that this gas might

FIG. 2.



A near view of a fractionating bulb in Sir William Ramsay's laboratory. The fractionating bulb *A* is immersed in liquid air in a Dewar tube, and the outline of the bulb *A* is indicated by the frozen xenon inside the bulb.

be argon, and so obtained a sample of the mineral, heated it with sulphuric acid, pumped off the evolved gas, sparked it with oxygen, and finally ran it into a spectrum tube. The spectrum was entirely different from that of argon, having as a chief characteristic a bright yellow line. According to Ramsay's own account to the writer, his surprise was so great that he at first thought there was something wrong with the spectroscope, and took out his handkerchief and wiped the prism. In addition, others suggested various explanations for the bright yellow line, the most common

being that it was the sodium line due to dirty electrodes or other causes. It was only when the spectrum of the sodium flame was compared through a comparison prism with the yellow line from the spectrum tube that it became evident beyond question that a new element—namely, terrestrial helium—had been discovered. In 1868 Janssen had noticed this brilliant yellow line in the spectrum of the sun. The name "helium" was given to the element that was responsible for this line by Franklin Lockyer. Sir William Crookes measured the wave-length of the line obtained from the tube first used by Ramsay, and it was found to have a wave-length identical with that of the helium line in the sun. An announcement of the discovery was made at the meeting of the Chemical Society in March, 1895.

It was now probable that other gases besides helium and argon existed. After a large number of minerals had been examined, in addition to the gases from mineral waters, as well as sea-water, which were investigated with the object of finding all possible sources of argon and helium, as well as for new gases, Ramsay, assisted by his pupil, Travers, decided on the fractionation of a considerable quantity of argon. Fifteen litres of the latter gas, as already described, had been prepared, and this was used for the investigation. Before it was fractionated, however, a litre of liquid air presented by Doctor Hampson was allowed to evaporate and the last fraction of gas collected separately. The spectrum of this gas, after purification from nitrogen and oxygen, showed a bright green and a bright yellow line not belonging to the argon spectrum. The density of the sample was also greater than that of argon. The new element was called krypton.

It was natural that with this encouragement the work should be carried on with great eagerness, for there was promise of still more to come. After the discovery of all the gases, and during the examination of their properties, work was continued night and day, only such rest being taken as was absolutely necessary. Such a fruitful field was bound to attract competition; and others, with even a better equipment than Ramsay had, attempted to take advantage of the results of his labor.

With the assistance of Doctor Travers, the 15 litres of argon referred to above was now fractionated. The first fraction which came off, containing the gases with the lowest boiling-points, was

separated and examined. The spectrum showed a large number of brilliant red and yellow lines, and the density of this particular sample was about 15. This new gas was called neon. Apparatus was now devised in connection with a Hampson liquid air machine, with which the lighter fractions from liquid air could be reliquefied and refractionated. Finally, a portion was obtained which could not be liquefied even by air, boiling under reduced pressure. This proved to consist largely of neon, but also contained helium—prov-

FIG. 3.

Photograph of Sir William Ramsay, taken in his laboratory. The Töpler pump, before which he is standing, is the one that he used in all of his rare-gas work.

ing the existence of the latter gas in the air. All attempts to separate the helium from the neon were futile, until, later on, the two were fractionated at the temperature of liquid hydrogen. At this temperature the neon solidified and the helium could be pumped off.

The residues, or last fractions, from 30 litres of liquid air, which had been allowed to evaporate, were now used to make a larger quantity of krypton. A majority of the gas consisted of argon, which was carried through a rigorous course of fractiona-

tion, the last fractions, containing the krypton, showing some new blue lines, with a density greater than that of krypton. After careful refractionation, the final gas obtained gave a density of 64, and the new element was called xenon. The density put this gas in its proper place in the Periodic Table. The volume obtained was only 3 c.c., while the total volume of krypton recovered was 12 c.c. All of the physical constants of these gases were afterward found with the small volumes indicated, and only men possessed of the manipulative skill of Ramsay and Travers could have done this.

The original densities found for these gases were:

Helium	1.98
Neon	9.97
Argon	19.96
Krypton	40.88
Xenon	64

The ratio of specific heat at constant pressure to that at constant volume indicates that all of the gases are monatomic. Their atomic weights, therefore, are:

Helium	3.96
Neon	19.94
Argon	39.92
Krypton	81.76
Xenon	128

Although more recent determinations by some of Ramsay's pupils have changed the atomic weights to some extent, particularly those of krypton and xenon, which are now taken at 92.92 and 130.22, respectively, the original determinations are exceedingly good, considering the conditions under which the work was done.

All attempts made by Ramsay and his pupils to get the rare gases to combine either with each other or with other elements were a failure. Argon was sparked with oxygen, chlorine, and hydrogen. Red-hot sodium hydroxide, sodium peroxide, or fused potassium nitrate had no action. Fluorine, nascent chlorine, and red-hot magnesium were equally ineffective. Helium and the other gases appeared to be as inactive as argon.

The original proportions of the rare gases in the atmosphere determined by Ramsay are, by volume:

Helium	1 part in 245,000
Neon	1 part in 81,000
Argon	1 part in 106.8
Krypton	1 part in 20,000,000
Xenon	1 part in 170,000,000

More recent determinations have modified some of these figures slightly, but it is remarkable that the original determinations were as accurate as they have proved to be.

For a number of years Ramsay devoted his attention to other lines of research; further work was done on the rare gases in his laboratory in 1907 and 1908. The probable presence of nebulium in the nebulæ and coronium in the sun made him anxious to investigate more fully the possibility of other rare gases in the atmosphere, either lighter than helium or heavier than xenon. The opportunity came in 1907, when Monsieur Claude, of Paris, offered to furnish a considerable amount of residue obtained in the preparation of oxygen on a large scale by the Claude process. Under the conditions of fractionation of nitrogen from oxygen, the heavier constituents of the atmosphere, such as krypton and xenon, would very largely be left with the liquid oxygen; and, as this oxygen was fractionated off, the residue obtained at the end of any considerable period would retain most of the krypton and xenon and any heavier gas that might be present. One of Ramsay's pupils at first fractionated the residue obtained from 20 tons of liquid air, and the final fractions were examined by the spectrum for new lines without result. As the gases had been subjected to contact with water, there was a possibility of any new gas being dissolved, and hence the work was repeated, using the residue from 100 tons of liquid air, the fractionation being carried out almost entirely without the use of water. The last fraction obtained from the fractionation of the xenon was examined very carefully with the spectroscope, but proved to be pure xenon—no additional lines being present. The result of the work showed that if a new gas, heavier than xenon, existed in the air, it was present in a smaller amount than one part in 2,560,000,000. About 250 c.c. of xenon was obtained in this work, and a slightly smaller amount of krypton, the new material being used for redetermining the densities of the gases. This

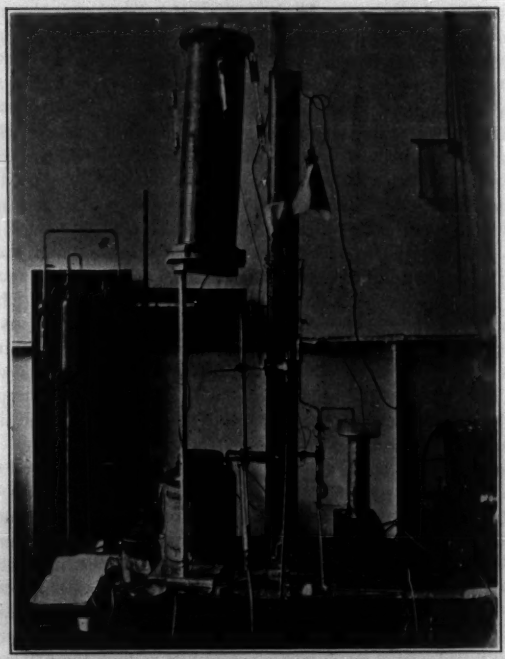
is the only occasion on which krypton and xenon have ever been prepared in quantity. About the same time Watson worked with Ramsay on the lighter gases, hoping to obtain a gas with smaller density than helium. This search was also unsuccessful, although it resulted in a number of new lines being added to the helium spectrum.

The discovery of radium and its disintegration products opened a new field not only to the physicist, but also to the chemist. The early work was done mainly by physicists, and its progress was therefore hampered for several years. Such a field was bound to be attractive to Ramsay, whose fertile mind quickly saw the possibilities for scientific chemical work in connection with the new science. Doctor Soddy, who had been working with Professor Rutherford in Montreal, came to University College at Ramsay's invitation.

Rutherford had shown that the alpha particle was of a mass comparable to that of the helium atom, and he indicated his belief that the alpha particle actually was a helium atom. Ramsay and Soddy attempted to obtain the spectrum of the radium emanation, and devised an apparatus for handling the very small amount of gas that could be obtained from the radium at his disposal. The emanation was liberated by the solution of the radium salt in water, and, after purification from hydrogen, oxygen, and carbon dioxide, the small volume of resulting gas was run into a capillary tube, a platinum wire acting as one electrode, and the mercury column which confined the gas acting as the other electrode. Owing to slight impurities, they were unable to obtain a spectrum of the emanation, but, on standing, the helium spectrum gradually appeared. This result not only confirmed experimentally Rutherford's theory, but it was the first actual visual proof of the transmutation of one element into another, and gave many scientific men who had been doubting the results obtained by those working in radio-activity the concrete evidence that they desired. This experiment and its result were also something that the public could understand, and added new interest to the remarkable series of discoveries that had already been made in connection with radio-activity. Whereas this first attempt at obtaining the spectrum of the emanation was unsuccessful, later on Ramsay and Collie, working with a larger amount of radium, were successful in a renewed attempt.

The energy given out by radio-active substances, especially those of short life, is enormous. Ramsay argued that if the radio-active substances were changing spontaneously into other elements, the energy available in such changes might be used for

FIG. 4.



A corner of Sir William Ramsay's laboratory. The equipment shown is for the purpose of causing spectrum tubes to glow without actually passing a current between the electrodes.

transforming other non-radio-active elements into elements of lower atomic weight. With this idea in mind, he started on an investigation which has probably given rise to more discussion than any of his other work. Assisted by his pupil, Mr. A. T. Cameron, he found that if a solution of copper nitrate were

treated with radium emanation, not only was lithium found in the solution, but the gas which was afterward pumped from the bulb, consisting mostly of hydrogen and oxygen, from the decomposition of the water, after proper purification, gave the argon spectrum, but showed no helium lines. The gas, however, contained 0.34 c.c. of nitrogen. When distilled water was treated with the emanation, neon, with a trace of helium, was obtained, but no argon. The " water-gas " contained 0.307 c.c. of nitrogen. The contention of Ramsay and Cameron was that under certain conditions not only helium but some one of the other rare gases of the atmosphere might constitute one of the disintegration products of the radium emanation. Some experiments of Rutherford afterward proved that the neon from $^1/_{10}$ c.c. of air could be detected under similar conditions. This might be used as an explanation of the presence of neon in the second experiment, but did not explain the absence of helium and also of neon in the first. The nitrogen in both cases was about the same, and, therefore, if this were due to a leak, helium and neon should be recognized in both cases. Madame Curie repeated the work in which lithium was obtained from copper, and reported a negative result. This whole work was left in rather an unsatisfactory condition. The writer asked Ramsay, some time after the above results were published, whether he would repeat them, and received the reply:

" No, I do not think it advisable; I should only find lithium and neon again, and a duplication of results by me would not strengthen the evidence. I shall leave the work to some one else to repeat."

This reply was characteristic of Ramsay's attitude in scientific research. He was more concerned in blazing the trail than in working out details: these he left to others with less imagination and less resourcefulness. During the last few years—thanks to the American carnotite deposits—much more radium is available for such work, and it is to be hoped that some one with the necessary skill and with a much larger amount of radium than Ramsay had in his possession will repeat his work under more ideal conditions. Ramsay's theory was that when an alpha particle struck a non-radio-active atom a glancing blow near the surface the atom was ionized; if it struck the atom squarely in the centre, the latter was broken up with the formation of new elements. Whether this theory will ultimately prove to be cor-

rect or not, the work done by Ramsay and Cameron was extremely suggestive and valuable.

At the same time that the above experiments were being carried on, Ramsay and Cameron were investigating the effect of the radium emanation on other chemical reactions. They showed that the emanation decomposed water and caused hydrogen and oxygen to recombine. Hydrochloric acid gave hydrogen and chlorine; mercury absorbed the chlorine, forming a deposit of mercurous chloride. Ammonia was decomposed and the nitrogen and hydrogen of these gases recombined; the amount of recombination (measured by absorbing the ammonia with calcium chloride) was smaller for the same quantity of emanation than the decomposition. Carbon monoxide gave carbon, oxygen, and carbon dioxide. The quantitative results indicated that the amount of chemical action taking place at any time is strictly proportional to the amount of emanation which decays in that time. From this it can be deduced that, other conditions being the same, each atom of emanation as it disintegrates produces a certain definite chemical effect.

In 1910 Ramsay, assisted by Dr. Whytlaw Gray, carried on what was probably the most difficult piece of chemical experimental work ever done. According to the disintegration theory, the radium emanation atom is formed from the radium atom by the elimination of the α particle or helium atom. If radium has an atomic weight of 226 and helium an atomic weight of 4, radium emanation should have, theoretically, an atomic weight of 222. Ramsay and Gray started out to test this theory experimentally. The amount of emanation they had to work with at any given time was not more than 0.005 cu. mm. In order to determine the atomic weight, it was necessary to weigh this minute volume of gas. Fortunately, a suitable balance was at hand in the form of a modification of the Steele and Grant microbalance, which was sensitive to one two-hundred-thousandth of a milligramme. The skill necessary to collect, purify, and weigh the very small volumes of emanation available taxed Ramsay's capabilities probably more than any other piece of work he ever attempted. But the result more than justified the effort. The mean of five determinations gave the atomic weight of the emanation as 223, a striking experimental proof of the theoretical value.

Ramsay carried the enthusiasm which he showed in his scientific work into other lines of endeavor. No matter what he became interested in, he gave his whole heart to the work in hand; and these interests were as broad as they were varied.

He took an active part in the reorganization of the University of London. The old University was an examining body, pure and simple. All that a student had to do was to appear for the examinations and pass them in regular order to obtain his degree. This arrangement did not meet with Ramsay's approval. He felt that, whereas examinations were useful and important, they should not be the chief means of testing a man's ability—hence he threw the whole weight of his influence into the scale for a teaching university, where a man's regular work would count in addition to examinations.

No one knew better than he the value of real productiveness on the part of a student; hence in his own laboratory he encouraged his men to start research work as early as possible, usually in that period of their course that would correspond to the senior year in an American university. In this way the more advanced undergraduates came in direct contact with him and received the benefit of his personal advice and influence, although they always actually worked under instructors or other research students.

Ramsay was an excellent linguist, speaking French and German fluently, and having a knowledge of several other languages. The writer well remembers his interest at times in the Chinese tongue, and, although he never became proficient in this language, the fact that he was willing to put even some time on it shows the interest that he took in matters of this kind. When he gave lectures in France and Germany he was always able to give them in the native tongue of his audience.

One of his chief personal characteristics was his enthusiasm, and this he was able to impart to those around him and to those who worked under him. His students were devoted to him at all times, and the inspiration they received from even a short contact with him was enough to last them a lifetime. It was seldom that he lost patience even with poor work—his plan being to lead and not to drive. Those who "made good" became his friends, and he kept in touch with them long after they had left his immediate influence. His letters always contained references to members of his faculty and students who had left, telling

where they had gone and what they were doing. Up to his last years he carried on his heavy correspondence without the aid of a secretary. In a letter, under date of July 23, 1910, he said:

"It is always a pleasure to hear from you. Many thanks for yours of a month ago. My correspondence has grown so formidable that I can hardly keep going; I don't quite know what to do to relieve the strain."

And in another letter he wrote:

"I have been owing you a letter for long. It has sat like a weight on my vitals, and now I am going to disgorge. You have given me an emetic—that is not very complimentary to your letter I fear, but I suppose there *are* pleasant emetics."

His first work at University College for many years was to give a lecture on general chemistry from 9 to 10 in the morning. After this lecture he went to his private laboratory and attended to his correspondence, which often took him until noon. The afternoon was given up to superintending the work of the research men in the laboratory and to actual experimental work on his own part when he did not have affairs of moment outside that called him away.

Ramsay's courage was another great asset. He was never afraid to say or write what he believed to be true, no matter what criticism might be aroused. On one or two occasions he made premature announcements that had to be taken back, but these were few and far between and need not be remembered in connection with the magnificent series of actual discoveries that he gave to the world. It was this quality of courage more than anything else that made him the great man that he was. He had confidence in his own work, and when his results were contrary to those of others he was not afraid to stand by them. On the other hand, he was equally willing to acknowledge a mistake when it was made.

In all of his work he was little interested in details. He was a pioneer in every sense of the word, and after he had beaten out a trail for others to follow he was ever willing to start a new one in some other direction. All of his investigations, especially during the last twenty years of his life, dealt with fundamental problems in chemistry. He was not a great theorist, his work being almost entirely along experimental lines; and when his practical results differed with theory, he was inclined to believe

that there was something wrong with the theory. His skill as an experimenter justified this attitude, which was partly responsible for his numerous great discoveries.

Socially he was a charming man, and lacked a great deal of the reserve that Americans associate with most Englishmen. He was full of fun and appreciated at all times a good joke. His home was the scene of many charming entertainments and dinners, and no one knew Ramsay fully who did not see him at his own fireside with his family.

Although a tremendously hard worker, he believed in a reasonable amount of recreation, and usually spent part of either the Christmas or summer vacation in travel. In a letter, written in 1908, he said:

"We have had a very good holiday; first to France, then to Skye, then Clyde, then Dublin, then Inverary, and, lastly, the English Lakes. It looks a century since we left London. Now I have been interviewing students all day. We are in great embarrassment trying to fit 130 people into 100 places. I believe it cannot be done without destruction of life and using the lockers as coffins."

And again, in 1911, he wrote:

"We have just come back, first from Algiers, where we spent a week; next from Paris, where we spent 10 days, being feasted twice a day. It was great fun, and it was delightful meeting such a lot of old friends and making some new ones. Among the latter was Urbain, who is a very strong man. His rare-metal work is admirable. I also saw the fourth Becquerel; his great-grandfather occupied the same laboratory as he does, and he is actually continuing the work of the family, and doing it magnificently—measuring the Zeemann effect on crystals. A lot of work is going on, and the workers are delightfully enthusiastic."

There is no doubt that some of these workers obtained a portion of their enthusiasm from Ramsay himself, even though the personal influence was for only a short time. Ramsay was always interested in public questions of moment, and his opinion was always worth while. He was broad-minded and tried to look at every question from different angles so as to get a proper perspective. At one time, when the liquor question was uppermost in the minds of the English people, he discussed the matter fully with the writer, and proposed a solution that seemed espe-

cially applicable to the English problem where high alcoholic beer and stout are consumed instead of whiskey. His suggestion was that a graduated internal revenue tax be put upon liquor of various kinds, such tax being proportionate to the alcoholic content, the tax being increased at a very rapid rate as the alcoholic content increased. In other words, he desired to not only encourage but force the English brewers to make lighter beers similar to those made in Germany, instead of the heavy English ales. He wrote on various timely topics, especially after the war began, when he furnished a series of articles to one of the Manchester papers that dealt with some of the problems that confronted the English nation in an extremely forceful manner. He felt very strongly concerning the unpreparedness of his nation, and did not hesitate to express his opinions where he believed efficiency was lacking. Under date of July 24, 1915, he wrote:

"Things are drifting along, and it appears that the war will be a long one. We are accumulating men and munitions, and there is some talk of forcing the pace before long. You may have heard that I have been doing my level best to stop our 'fools' of ministers from supplying Germany with cotton; it looks as if we were about to succeed at last, but the war would have been over before this if they had done what I asked in January; indeed, they were pressed since September to make cotton contraband."

Again, in another letter of the same year, he said:

"I am also trying to induce our Government to make cotton contraband. In 1913 Germany and Austria imported 670,000 tons of cotton. To-day, so far as conservative estimates can be made, the consumption for war purposes is 350,000 tons a year. If copper is contraband, why not cotton?"

Although no English chemist was more honored by the Germans than Ramsay, there was no English chemist that was more loyal to his country when the break came. Concerning the war, he wrote:

"I have before me your letter of June 11 of last year. What a lot of things have happened since then! I knew Germany meant to attack us, but I thought 1915 would be the year. What I did not know was, first, that they are such brutes; I thought it would be a civilized war; and, second, that their plans would

miscarry so hopelessly. We have been awfully lucky—first, in the Belgians delaying the attack, and, second, in our having a Liberal government in power. If the conservatives had been in, the Liberals would have done all in their power to hinder. We were also lucky in Kitchener being available; the conservatives forced the Government to put him at the head of the War Office. Also, we were lucky in having the navy so ready and bottling up theirs. Their recent threat has already come to nothing."

And again:

"You Americans will have to bestir yourselves; are you aware that Germany is organized as a huge trust? You have found that trusts are inconvenient and have legislated against them. What if a nation becomes a trust? The French and Russians are recognizing this, and are considering very drastic measures. A commercial war (for Germany regards commerce as a war, while we look on it as designed for mutual convenience) can only be met by a species of war; and Russia and France are thinking of ostracising Germans and German trade absolutely, buying up all German concerns in Russia and France and excluding all Germans and their goods. The only way of bringing home to Germany the error of their conception of trade is to show them that it will not pay. Well, you will have to consider your position, big as you are; if European markets are closed to Germany and American ones are open, you will have 'dumping' on an immense scale if the Germans can afford it. Indeed, I think you will have a prodigious influx of Germans, for the 'Fatherland' will be a most undesirable place of residence when war is over. The utmost farthing of indemnity will be exacted, and Russia and France will not spare them, whatever we do.

"This letter treats too much of the war, but it is in all our thoughts. You might treat me handsomely and not wait as long as I have done before writing."

In another letter he wrote in the same strain:

"I am glad to see in to-day's papers that President Wilson has made such a 'stiff' answer to the Germans. It appears incredible that you Americans should turn the other cheek to the massacre of a large number of your citizens. But I suppose the German element in the States is strong, and that this informal vote is at the bottom of it. It is one of the disadvantages of a

republican government; it is all right in peace, but breaks down in war."

During his last few years Ramsay was engaged on a number of research problems which were either not completed or did not give positive results. Among these was an attempt to synthesize argon from sulphur and hydrogen by means of cathode rays; and the synthesis of krypton from selenium and hydrogen. Also, he investigated the spectrum of radium emanation as it decayed. Some results which he obtained in the action of the radium emanation on thorium nitrate, in which carbon dioxide was always formed, led to an extension of this work to other compounds, such as bismuth and lead perchlorates and chlorates. Ramsay was never held back from trying something on account of the improbability of obtaining a positive result. He never gave up until negative results accumulated sufficiently to prove the futility of further work along the same lines.

After resigning his active work in connection with University College, he retired to a country home, a short distance from London. Concerning this, he wrote:

"We have bought a house about 35 minutes from London by fast train at a little village named Hazlemere, near High Wycomb. The house is ugly but commodious. There are about five acres of ground, mixed wood and meadow, and a capital garden. I am adapting the stage- and coach-house as a laboratory. I doubt if we shall shift before the end of February; the alterations will take some time. We shall have to make our own electricity and gas, but nowadays this is easy.

"I am finding it a great relief not to have these 9 o'clock lectures. I feel as if I were having a perpetual holiday, with plenty to do, but no necessity to do anything at a given moment."

It was here that his last illness seized upon him, and here that he fought his last battle against the dreadful disease that he had done some work to combat during his lifetime. The courage that he showed in his work and in all his undertakings during his lifetime remained with him to the end.

Ramsay received many honors, but none that he did not deserve. He was made a Knight Commander of the Bath, a Knight of the Prussian Order, "Pour le Merite," an Officer of the Legion of Honor of France, and a Commander of the Crown of Italy. In 1904 he received the Nobel prize in chemistry; in

1895 the Davy Medal of the Royal Society, and in 1897 the Longstaff Medal of the Chemical Society. He was president of the Chemical Society in 1907, and received many honorary degrees from various universities, and was a foreign member of scientific societies in almost every foreign country.

Ramsay was great, not only on account of his scientific discoveries, but on account of his greatness of character, and this greatness was illustrated in no better way than by the service he did to humanity. He gave himself freely, and freely he was rewarded.

BIBLIOGRAPHY.*

"A New Method of Forming Orthotoluic Acid," with Rudolph Fittig, *Ann. Chem. Pharm.*, clxviii, 202.

"On m-Toluic Acid," with Carl Böttinger, *Ann. Chem. Pharm.*, clxviii, 253.

"Preliminary Note on Antimony Phosphide," *Deut. Chem. Ges. Ber.*, vi, 1362.

"Hydrogen Persulphide,'" *Jour. Chem. Soc.*, **27**, 857.

"Sodium Ethylthiosulphate," *Jour. Chem. Soc.*, **28**, 687.

"On Bismuthiferrous Tesseral Pyrites," *Jour. Chem. Soc.*, **29**, 153.

"Dehydration of Hydrates by the Time Method," *Jour. Chem. Soc.*, **32**, 395.

"On the Decomposition Products of Quinine" (first paper, "Oxidation with Permanganate," with J. J. Dobbie), *Jour. Chem. Soc.*, **33**, 102.

"On the Decomposition Products of Quinine, and the Allied Alkaloids" (second paper, "Oxidation of Quinine, Quinidine, Cinchonine, and Cinchonidine with Permanganate," with J. J. Dobbie), *Jour. Chem. Soc.*, **35**, 189.

"On the Volumes of Liquids at Their Boiling-points, Obtainable from Unit Volumes of Their Gases," *Jour. Chem Soc.*, **35**, 463.

"On the Heat of Formation of Aniline, Picoline, Toluidine, Lutidine, Pyridine, Dipicoline, Pyrrol, Glycerin, and Furfurol," *Jour. Chem. Soc.*, **35**, 696.

"On Picoline and its Derivatives," *Phil. Mag.*, October, 1876; October, 1877, and July, 1878.

"On the Volumes of Sodium, Bromine, and Phosphorus at Their Boiling-points," with D. O. Masson, *Jour. Chem. Soc.*, **39**, 49.

"On the Volumes of Some Compounds of the Benzene, Naphthalene, Anthracene, and Phenanthrene Series," *Jour. Chem. Soc.*, **39**, 63.

"On the Atomic Volume of Nitrogen," *Jour. Chem. Soc.*, **39**, 66.

"On the Critical Study of Gases," *Proc. Roy. Soc.*, **30**, 323; **31**, 194.

"The Halogen Compounds of Selenium," with F. P. Evans, *Jour. Chem. Soc.*, **45**, 62.

"The Decomposition of Ammonia by Heat," with S. Young, *Jour. Chem. Soc.*, **45**, 88.

* Papers by Sir William Ramsay's pupils, in connection with which his name does not appear, are not included in this bibliography.

"Determination of Boiling-points," with S. Young, *Proc. Chem. Soc.*, 1886, 181.

"Presence of a Reducing Agent, Probably Hydrogen Peroxide, in Natural Water," *Proc. Chem. Soc.*, 1886, 225.

"The Non-existence of Gaseous Nitrous Anhydride," with J. T. Crindall, *Jour. Chem. Soc.*, **47**, 672.

"The Oxides of Nitrogen," same authors, *Jour. Chem. Soc.*, **47**, 187.

"A Method for Obtaining Constant Temperatures," with S. Young, *Jour. Chem. Soc.*, **47**, 640.

"On a New Method of Determining the Vapor Pressure of Solids and Liquids and on the Vapor Pressure of Acetic Acid," with S. Young, *Jour. Chem. Soc.*, **47**, 42.

"Influence of Pressure on the Temperature of Volatilization of Solids," with S. Young, *Proc. Roy. Soc.*, **35**, 308.

"Influence of Change from Liquid to Solid State on Vapor Pressures," with S. Young, *Proc. Roy. Soc.*, **36**, 499.

"Thermal Properties of Ethyl Alcohol," with S. Young, *Proc. Roy. Soc.*, **38**, 329.

"Evaporation and Dissociation: A Study of the Thermal Properties of Acetic Acid," with S. Young, *Jour. Chem. Soc.*, **49**, 790.

"Note on the Vapor Density of Chloral Ethyl Alcoholate," with S. Young, *Jour. Chem. Soc.*, **49**, 685.

"On the Vapor Pressure of Bromine and Iodine and on Iodine Monochloride," with S. Young, *Jour. Chem. Soc.*, **49**, 453.

"On the Vapor Pressure of Mercury," with S. Young, *Jour. Chem. Soc.*, **49**, 37.

"So-called Specific Remission," with S. Young, *Ber.*, **18**, 2855.

"Do the Statical and Dynamical Methods of Measuring Vapor Pressure Give Identical Results?" A reply to G. W. F. Kahlbaum, with S. Young, *Ber.*, **19**, 69.

"Nature of Liquids as Shown by a Study of the Thermal Properties of Stable and Dissociable Bodies," with S. Young, *Proc. Chem. Soc.*, 1886, 226.

"The Statical and Dynamical Methods of Estimating Vapor Pressure," with S. Young, *Ber.*, **19**, 2107.

"A Study of the Thermal Properties of a Mixture of Ethyl Alcohol and Ethyl Oxide," with S. Young, *Jour. Chem. Soc.*, **51**, 755.

"Experiments for the Purpose of Comparing the Equivalent of Zinc with that of Hydrogen," with H. C. Reynolds, *Jour. Chem. Soc.*, **51**, 854.

"The Continuous Transition from the Liquid to the Gaseous State of Matter at all Temperatures," with S. Young, *Phil. Mag.*, **23**, 435.

"Influence of Change of Condition from the Liquid to the Solid State on Vapor Pressure," with S. Young, *Phil. Mag.*, **23**, 61.

"Nature of Liquids as Shown by a Study of the Thermal Properties of Stable and Dissociable Substances," with S. Young, *Phil. Mag.*, **23**, 129.

"Nature of Liquids," with S. Young, *Chem. News*, **54**, 203.

"Thermal Properties of Ether," with S. Young, *Proc. Roy. Soc.*, **40**, 381.

"Thermal Properties of a Mixture of Ethyl Alcohol and Ethyl Oxide," with S. Young, *Jour. Chem. Soc.*, **52**, 755.

"The Molecular Weights of Nitrogen Trioxide and Nitrogen Peroxide," *Jour. Chem. Soc.*, **53**, 621.

"Evaporation and Dissociation: Continuous Change from the Gaseous to the Liquid State at All Temperatures," with S. Young, *Phil. Mag.*, **24**, 196.

"The Molecular Weights of the Metals," *Jour. Chem. Soc.*, **55**, 521.

"Mixture of Propyl Alcohol and Water," with S. Young, *Proc. Chem. Soc.*, 1888, 101.

"Nitrogen Trioxide and Peroxide," *Jour. Chem. Soc.*, **57**, 590.

"Dissociation of Selenium Chloride," *Bull. Soc. Chim.*, **3**, 783.

"Some Ideas about Solution," *Zeit. physikal. Chem.*, **7**, 511.

"Nepheline-Syenite of the Kola Peninsula," *Jahrb. f. Min.*, 1891, i, Ref. 97–99.

"Pedetic Motion in Relation to Colloidal Solutions," *Proc. Chem. Soc.*, 1892, 17.

"Rock of the Jiwaara, in Finland," with H. Berghill, *Jahrb. f. Min.*, 1892, i, Ref. 307–308.

"Note on the Combination of Dry Gases," *Proc. Chem. Soc.*, 1893, 165.

"The Atomic Weight of Boron," with Miss Ashton, *Jour. Chem. Soc.*, **63**, 207.

"The Boiling-point of Nitrous Oxide at Atmospheric Pressure and the Melting-point of Solid Nitrous Oxide," with J. Shields, *Jour. Chem. Soc.*, **63**, 833.

"The Molecular Complexity of Liquids," with J. Shields, *Jour. Chem. Soc.*, **63**, 1089.

"The Molecular Formulæ of Some Liquids, as Determined by Their Molecular Surface Energy," with Miss E. Ashton, *Jour. Chem. Soc.*, **65**, 167.

"Variation of Molecular Surface Energy and Temperature," with J. Shields, *Phil. Trans.*, **184**, 647.

"The Critical State," *Zeit. physikal. Chem.*, **14**, 486.

"Discovery of Helium in Clevite," *Jour. Chem. Soc.*, **67**, 1107.

"Passage of Hydrogen Through a Palladium Septum," *Phil. Mag.*, **38**, 206.

"Complexity and Dissociation of Liquid Molecules," *Proc. Roy. Soc.*, **56**, 171, and *Zeit. physikal. Chem.*, **15**, 106.

"Molecular Surface Energy of Mixtures of Non-associating Liquids," with Miss E. Ashton, *Zeit. physikal Chem.*, **15**, 89.

"Molecular Surface Energy of Ethereal Salts," with Miss E. Ashton, *Proc. Roy. Soc.*, **56**, 162.

"Gas Showing the Spectrum of Helium," *Proc. Roy. Soc.*, **58**, 65.

"Helium, a Gaseous Constituent of Certain Minerals" (Part I), *Proc. Roy. Soc.*, **58**, 81.

"Helium, a Constituent of Certain Minerals," with J. Norman Collie and M. W. Travers, *Jour. Chem. Soc.*, **67**, 684.

"Pressure, Volume and Temperature Relations of Rarefied Gases," with E. C. C. Baly, *Phil. Mag.*, **38**, 301.

"Occlusion of Oxygen and Hydrogen by Platinum Black," with L. Mond and J. Shields, *Proc. Roy. Soc.*, **58**, 242, and *Zeit. anorg. Chem.*, **10**, 178.

"Helium, a Gaseous Constituent of Certain Minerals" (Part II, "Density"), *Proc. Roy. Soc.*, **59**, 325.

"Argon, a New Constituent of the Atmosphere," with Lord Rayleigh, *Phil. Trans.*, **186**, 187.

"A Possible Compound of Argon," *Chem. News*, **72**, 51.

"Homogeneity of Argon and Helium," with J. N. Collie, *Compt. Rend.*, **123**, 214, and *Nature*, **54**, 546.

"Behavior of Argon and Helium when Submitted to the Electric Discharge," with J. N. Collie, *Proc. Roy. Soc.*, **59**, 257.

"Examination of Gases from Certain Mineral Waters," with A. Kellas, *Proc. Roy. Soc.*, **59**, 68.

"Method of Comparing Directly the Heats of Evaporation of Different Liquids at Their Boiling-points," with Dorothy Marshall, *Phil. Mag.*, **41**, 38.

"Helium and Argon" (Part III, "Experiments Which Show the Inactivity of These Elements"), *Proc. Roy. Soc.*, **60**, 53.

"Homogeneity of Helium and Argon," with J. N. Collie, *Proc. Roy. Soc.*, **60**, 206.

"Attempt to Determine the Adiabatic Relations of Ethylic Oxide," with E. P. Perman and J. Rose Innes, *Proc. Roy. Soc.*, **60**, 336.

"Gaseous Constituents of Certain Mineral Substances and Natural Waters," with M. W. Travers, *Proc. Roy. Soc.*, **60**, 442.

"Attempt to Cause Helium or Argon to Pass Through Red-hot Palladium, Platinum, or Iron," with M. W. Travers, *Proc. Roy. Soc.*, **61**, 267.

"Refractivities of Air, Oxygen, Nitrogen, Argon, Hydrogen, and Helium," with M. W. Travers, *Proc. Roy. Soc.*, **62**, 225.

"New Atmospheric Gases," with M. W. Travers, *Compt. Rend.*, 126, 1762.

"Occlusion of Oxygen and Hydrogen by Platinum Black," with L. Mond and J. Shields, *Proc. Roy. Soc.*, **62**, 50.

"Occlusion of Hydrogen and Oxygen by Palladium," with L. Mond and J. Shields, *Proc. Roy. Soc.*, **62**, 290.

"Monazite from Finland," with A. Zilliaeus, *Zeit. Kryst. Min.*, **31**, 317.

"The Newly Discovered Gases and Their Relation to the Periodic Law," *Ber.*, **31**, 3111.

"Homogeneity of Helium," with M. W. Travers, *Proc. Roy. Soc.*, **52**, 316.

"Fergusonite, an Endothermal Mineral," with M. W. Travers, *Proc. Roy. Soc.*, **62**, 325.

"Densities of Atmospheric Nitrogen, Pure Nitrogen, and Argon," *Proc. Roy. Soc.*, **64**, 181.

"Preparation, and Some of the Properties of Pure Argon," with M. W. Travers, *Proc. Roy. Soc.*, **64**, 183.

"Specific Gravities of the Halogens at Their Boiling-points, and of Oxygen and Nitrogen," with J. Drugman, *Jour. Chem. Soc.*, **77**, 1228.

"Note on the Supposed Formation of an Oxide of Hydrogen Higher than the Dioxide," *Jour. Chem. Soc.*, **79**, 1324.

"Action of Heat on Ethylsulfuric Acid," with George Rudorf, *Proc. Chem. Soc.*, 1900, 177.

"Preliminary Note on Hydrides of Boron," with H. S. Hatfield, *Proc. Chem. Soc.*, 1901, 152.

"Refractivity of the Inactive Gases," *Arch. Néerland, Sci. Exact. Nat.*, **5**, 356.

"Argon and its Companions," with M. W. Travers, *Proc. Roy. Soc.*, **67**, 329.

"Calorimetric Method for Estimating Oxygen Dissolved in Water," with Ida Homfray, *Jour. Soc. Chem. Ind.*, **20**, 1071.

"Etching Fluid for Micro-metallurgy," *Chem. News*, **87**, 291.

"Attempt to Estimate the Relative Amount of Krypton and Xenon in Atmospheric Air," *Proc. Roy. Soc.*, **71**, 421.

"Molecular Surface Energy of Some Mixtures of Liquids," with Miss Emily Ashton, *Trans. Roy. Irish Acad.*, **32**, 93.

"Chemical Behavior of Gutta-percha," with Miss Harriette Chick and Frank Collingridge, *Jour. Soc. Chem. Ind.*, **21**, 1367.

"Experiments in Radioactivity and the Production of Helium from Radium," with Frederick Soddy, *Proc. Roy. Soc.*, **72**, 204.

"Vapor Densities of Some Carbon Compounds: An Attempt to Determine Their Exact Molecular Weight," with Bertram D. Steele, *Zeit. physikal. Chem.*, **44**, 348.

"Meteorite of Bjurböle," with Leon H. Borgström, *Bull. Comm. Géol. Finlande*, **12**, 1–28.

"Properties and Changes of Radium Emanation," *Compt. Rend.*, **138**, 1388.

"Occurrence of Thorium in Ceylon," *Nature*, **69**, 533, 559.

"Spectrum of Radium Emanation," with John Norman Collie, *Proc. Roy. Soc.*, **73**, 470.

"Further Experiments on the Production of Helium from Radium," *Proc. Roy. Soc.*, **73**, 346.

"Decomposition of Water by Radium," *Meddel. K. Vet. Akad. Nobelinst.*, **1**, 909.

"A New Element, Radiothorium, the Emanation of Which is Identical with that of Thorium," *Jour. Chim. Phys.*, 1905, iii, 617.

"Determination of the Amounts of Neon and Helium in Atmospheric Air," *Proc. Roy. Soc.*, **76**, 111.

"Chemical and Electrical Changes Indiced by Ultra-violet Light," with J. F. Spencer, *Phil. Mag.*, **12**, 397.

"The Chemical Action of the Radium Emanation" (Part I, "Action on Distilled Water"), *Jour. Chem. Soc.*, **91**, 931; *Proceedings*, 1907, 132.

"Some Properties of Radium Emanation," with A. T. Cameron, *Jour. Chem. Soc.*, **91**, 1266; *Proceedings*, 1907, 178.

"The Chemical Action of Radium Emanation" (Part II, "On Solutions Containing Copper and Lead and on Water"), *Jour. Chem. Soc.*, **91**, 1593; *Proceedings*, 1907, 217.

"Selenium Hexafluoride," *Compt. Rend.*, **144**, 1042.

"The Chemical Action of the Radium Emanation," with A. T. Cameron (Part III, "On Water and Certain Gases"), *Jour. Chem. Soc.*, **93**, 966.

"The Chemical Action of Radium Emanation" (Part IV, "On Water"), with A. T. Cameron, *Jour. Chem. Soc.*, **93**, 992; *Proceedings*, 1908, 133.

"Percentage of the Inactive Gases in the Atmosphere" (a correction), *Proc. Roy. Soc.*, **80**, 599.

"Search for Possible New Members of the Inactive Series of Gases," *Proc. Roy. Soc.*, **81**, 178.

"Lithium in Radio-active Minerals," with A. T. Cameron, *Compt. Rend.*, **146**, 456.

"Spectrum of Radium Emanation," with A. T. Cameron, *Proc. Roy. Soc.*, **81**, 210.

"Some Physical Properties of Radium Emanation," with R. W. Gray, *Jour. Chem. Soc.*, **95**, 1073; *Proceedings*, 1909, 161.

"Liquid and Solid Radium Emanation," with R. W. Gray, *Proc. Chem. Soc.*, 1909, 82.

"Instability of Radium Bromide," *Monatsh.*, **29**, 1013.

"Action of Radium Emanation on the Elements of the Carbon Group," with F. L. Usher, *Ber.*, **42**, 2930.

"The Half-life Period of Radium" (a correction), with R. W. Gray, *Jour. Chem. Soc.*, **97**, 185.

"The Density of the Radium Emanation," with R. W. Gray, *Compt. Rend.*, **151**, 126.

"Action of Niton on Thorium Salts," *Compt. Rend.*, **153**, 373.

"The Density of Niton, and the Disintegration Theory," with R. W. Gray, *Proc. Roy. Soc.*, **84**, 536.

"The Formation of Neon as a Product of Radio-active Change," *Jour. Chem. Soc.*, **101**, 1367.

"An Analysis of the Waters of the Thermal Springs of Bath," with James Orme Masson, *Jour. Chem. Soc.*, **101**, 1376.

"Monatomicity of Neon, Krypton, and Xenon," *Proc. Roy. Soc.*, **86**, 100.

"The Mineral Waters of Bath," *Chem. News*, **105**, 133.

"Atomic Weight of Radium," with R. Whytlaw Gray, *Proc. Roy. Soc.*, **86**, 270.

"The Presence of Helium in the Gas from the Interior of an X-ray Bulb," *Jour. Chem. Soc.*, **103**, 264.

"A Hypothesis of Molecular Configuration on Three Dimensions of Space," *Jour. Roy. Soc.*, **92**, 451.

Coal Gas for Motor Traction. W. M. BARRETT. (*The Autocar*, vol. xxxix, No. 1, 148, p. 373, October 20, 1917.)—The restrictions placed by the British Government upon the use of petrol have aroused a great deal of interest during the last few months on the subject of coal gas for motor traction. In the early experiments gas compressed in steel cylinders was used, but, owing to difficulties experienced in reducing the pressure, a flexible gas bag carried on the roof of the vehicle was decided upon. Although many predictions of failure were made, the idea proved practicable and that method of storage soon became very popular. The gas container was made of woven fabric, treated so as to be gas-tight and waterproof. It was circular is shape and held in position on the roof of the vehicle by ropes. It was filled with gas through a flexible sleeve made of the same material as the container itself, and the connection to the engine was made of flexible rubber tube in which a control tap was fitted within easy reach of the driver's hand.

There were many practical disadvantages in using the circular-shaped container, not the least being the tendency of the container when partly deflated to droop over the side of the vehicle, unless the driver took a reef in the ropes every few miles, and the excessive chafing in consequence, to which the container was subject, has a bad effect upon its life. This led to the introduction of the D-shaped container. The bottom of this container is flat and firmly anchored to the superstructure. A further development is the semi-rigid container consisting of a gas-tight box with a flexible top. A development on entirely different lines is the collapsible container, which is made like a concertina, and is guided in its downward travel, as the gas is used up, by flexible stays running on rigid guide rods fixed perpendicularly to the roof of the vehicle, the guide rods being braced at the top to ensure rigidity. The top of the container is weighted with a thin sheet of metal, and as the gas is used up the container folds up and finally lies quite flat.

Much attention has been given to the question of compressing gas at high pressure in steel tanks for traction purposes. The greatest objections to steel cylinders are their weight, risk of accident from breakage while under high pressure, and depreciation of the gas by compression. Among various expedients to meet these objections a cylinder made of a composition of rubber, canvas, and other materials has recently been made by Wood-Milne, Ltd. This cylinder is 4 feet 6 inches long by 1 foot 3 inches in diameter. Weighing 60 pounds uncharged, this container is said to be capable of resisting an internal pressure of 1600 pounds per square inch. At 1000 pounds pressure each cylinder will contain 600 cubic feet of gas (the equivalent of over two gallons of petrol) and weighs, fully charged, less than 100 pounds.

PHYSICS OF THE AIR.*

BY

W. J. HUMPHREYS,

Professor of Meteorological Physics, United States Weather Bureau.

CHAPTER XIV (*Continued*).

Thunderstorm Winds.—Shortly, say 20 minutes or so, before the rain of a thunderstorm reaches a given locality the wind at that place, generally light, begins to die down to an approximate calm and to change its direction. At first it usually is from the south or southwest in the northern hemisphere; from the north or northwest in the southern, and in both more or less directly across the path of the storm itself. When the change is complete, it blows for a few minutes, rather gently, directly toward the nearest portion of the storm front, and finally, as the rain is almost at hand, abruptly and in rather violent gusts, away from the storm and, because it has come from above, as will be explained later, in the same direction that the storm is traveling, a direction that, in most cases, differs appreciably from that of the original surface wind. Usually this violent gusty wind lasts through only the earlier portion of the disturbance and then is gradually but rather quickly succeeded by a comparatively gentle wind, that though following the storm at first, frequently, after an hour or so, blows in the same general direction as the original surface wind.

The cause of the thunderstorm winds needs to be carefully considered if one would understand at all clearly the mechanism of the storm itself.

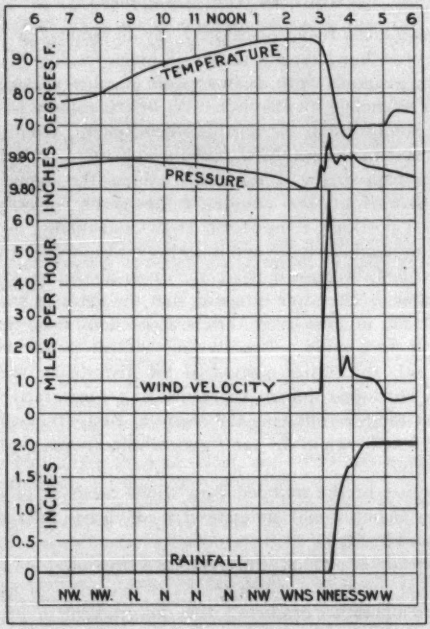
As already explained, this type of storm owes its origin to that vertical convection which results from a more or less superadiabatic temperature gradient. It is this gradient, no matter how established, whether by simple surface heating or by the over and under running of layers of air of widely different temperatures, that permits, or rather forces, the production of the cumulus cloud in which and by the motions of which the electricity that characterizes the thunderstorm is generated.

Nevertheless, as everyone knows, the passage of a cumulus

* Continued from page 824, vol. 185, June, 1918.

cloud overhead, however large, so long as no rain is falling from it, does not greatly affect the direction and magnitude of the surface wind—does not bring on any of the familiar gusts and other thunderstorm phenomena. Hence, somehow or other, the rain is an important factor, both in starting and in maintaining the winds

FIG. 102.



Course of meteorological elements on a thunderstorm day. (Washington, D. C., July 30, 1913.)

in question, for they do not exist before the rain begins nor continue after it has ceased. On the other hand, it cannot be assumed that the rain is the whole cause of these winds, for they do not accompany other and ordinary showers, however heavy the downpour may be.

The actual course of events, illustrated by Fig. 102, taken from

the records obtained at Washington, D. C., during the passage of the notable thunder squall of July 30, 1913, seems to be about as follows:

First.—An approximately adiabatic temperature gradient presumably is established over a wide area, roughly up to the base level of the cumulus clouds, all of which, because of a practically common temperature and common humidity over the whole region, must have substantially the same base level and therefore often appear *en échelon,* as shown in Fig. 103. But while the uprising branches of the existing convection currents, due to superadiabatic gradients, may be localized and here and there rather rapid, the return or compensating down-flow is relatively widespread and correspondingly gentle. The condition essential to a local and rapid down-flow, that is, a local decided cooling at a high altitude, does not exist, and therefore the counterpart to the upward current is nowhere conspicuous.

Second.—The convections in the cumuli are accelerated by virtue of the latent heat of vaporization set free in them, and thus one or more of them rapidly developed. In some cases great size and remarkable altitudes are attained, as illustrated by Fig. 104.

Third.—After a time, as a result of the abundant condensation induced by the convectional cooling, rain is formed at a considerable altitude where, of course, the air is quite cold, in fact so cold that often hail is produced. Now this cold rain, or rain and hail, as it falls, and as long as it falls, chills the air from the level of its formation all the way to the earth, partly as a result of its initial low temperature and partly because of the evaporation that takes place during its fall. Hence this continuously chilled column of air, because, partly, of the frictional drag of the rain, but mainly because of the increase, due to this chilling, of its own density, immediately and necessarily becomes a concentrated and vigorous return branch of the vertical circulation. In fact, it (or gravity acting through it) becomes the sustaining cause of the storm's circulation. At the same time, because of the downward blow and because of the retardation of the winds by surface friction, the barometric pressure is abruptly increased, as will be explained later.

It will be worth while to consider some of these statements a little more closely, and to test them with possible numerical values.

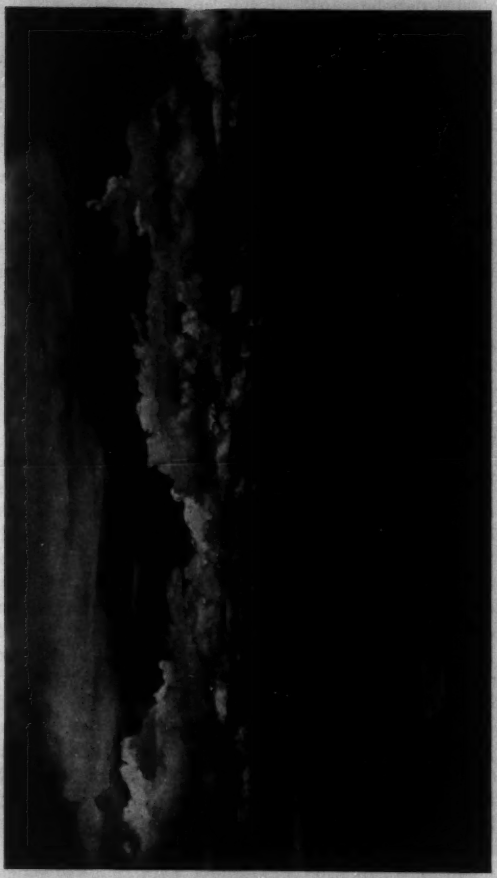


FIG. 103. Echelon clouds. (Loudon Valley, Va., E. B. Calvert, photo.)

Omitting, as one may, the effects of radiation, there seem to be but three possible ways by which the cooling of a thunderstorm may be obtained: (*a*) by the descent of originally potentially cold air; (*b*) by chilling the air with the cold rain; (*c*) by evaporation. Each of these will be considered separately.

(*a*) Obviously no portion of the upper air could maintain its position if potentially even slightly colder than that near the surface, that is, so cold that even after warming up adiabatically in

FIG. 104.

Towering cumulus. (West end of Java, E. E. Barnard, photo.)

a fall to the surface it still would be colder than the air displaced. If at all potentially colder it would fall until it itself became the surface air. Hence the great decrease in temperature that comes with a thunderstorm is not the result of the descent of a layer of air originally potentially cold for, as explained, an upper layer sufficiently cold to give, after its descent, the actual cooling could not exist. Again, any descending air must come from either below the under surface of the cloud or from above this level. If from

below, then, because of adiabatic heating during its descent through air which, as above explained, has practically the adiabatic temperature gradient, it must reach the earth at substantially the original surface temperature. If from above it would, as is obvious from Fig. 82, reach the earth even warmer than the original surface temperature. Hence, looked at in any way, case *a* clearly is inadmissible.

Possibly the above statements may seem to contravene the explanation that many thunderstorms originate in the establishment by cross currents of superadiabatic temperature gradients. In reality, however, they are in harmony with that explanation which is based on the fact that such gradients can not be maintained, but must at once cause vertical convection. Besides, such mechanically established gradients merely initiate but do not, as we shall see, maintain the storm.

(*b*) Let the under surface of the thunderstorm cloud be 1500 meters above the earth, and the column of air cooled by the cold rain and its evaporation, 2000 meters high. Let the surface temperature be 30° C., and the temperature gradient before the storm begins adiabatic up to the under-cloud level, and let there be a 2-centimeter rainfall.

Now at the temperature assumed, a column of air 2000 meters high whose cross section is 1 square centimeter, and whose base is at sea level, weighs, roughly, 210 grams, and its heat capacity, therefore, is approximately that of 50 grams of water. At the top of this column the temperature can be, at most, only about 20° C. lower than at the bottom, corresponding to the adiabatic or maximum temperature gradient, and if the rain leaves the top at this temperature but reaches the earth 7° C. colder than the surface air before the storm (temperatures that seem at least to be of the correct order), it will have been warmed 13° C. during its fall and the air column, at the expense of whose heat this warming was produced, cooled, on the average, about 0.5° C. But, as a matter of fact, the air usually is cooled from 5° C. to 10° C. Hence, while the temperature of the air necessarily is reduced to some extent by mere heat conduction to the cold rain, much the greater portion of the cooling clearly must have some other origin. Further, since *a* is inadmissible and *b* only a minor contributing factor, it follows by exclusion that of the three obvious causes only evaporation is left to account for much the greater portion of the

cooling. Consider, then, whether evaporation really can produce the effects observed.

(*c*) It is a common thing in semiarid regions to see a heavy shower, even a thunder shower, leave the base of a cloud and yet fail utterly to reach the surface of the earth. Hence, it appears quite certain that in the average thunderstorm a considerable portion of the rain that leaves the cloud may evaporate before it reaches the ground, and therefore that the temperature decrease of the atmosphere may largely be owing to this fact. But if so, why then, one properly might ask, does not an equally great temperature drop accompany all heavy rains?

The answer is obvious: It is because, as a rule, the temperature is higher, the relative humidity lower, and the temperature gradient more nearly adiabatic during a thunderstorm than at the time of an ordinary rain. Other rains, those that are accompanied by long horizontal and slow, rather than rapid upward movements of the air, begin only when the humidity is so high that but little evaporation and therefore but little cooling from this source can take place. In such rains there is nothing that can greatly increase the density of the air and consequently there is no rapidly descending current or wind. Thunderstorms, on the other hand, are developed by strong vertical convection which establishes a nearly adiabatic gradient and when the relative humidity, in the case of the heat thunderstorm, at least, is low, 50 per cent., say, evaporation into this air, as soon as the rain has begun, obviously must be rapid, with the consequent cooling and increase of density correspondingly great. Hence, since the temperature gradient was already nearly adiabatic, a strong downward current necessarily is established in the midst of the falling and evaporating rain. Further, whatever the type of thunderstorm, the descending air, which can be no more than saturated at the base of the cloud, dynamically warms so rapidly that evaporation into it can not keep pace with its water capacity. That is, evaporation which takes place all the way from cloud to earth, by rendering the air locally cool and dense, causes it to fall, while this fall, in turn, through dynamical heating, maintains the evaporation. Hence the downrush of the air must continue so long as there is an abundant supply of local rain, and cease when the rain becomes light.

It will be instructive now to return to the numerical values and compute a probable magnitude of the cooling due to evaporation.

As before, let a 2-centimeter rain leave the cloud, but let one-fourth of the rain that started, or half a centimeter, be evaporated. This would consume 303 heat units from an air column 2000 meters high whose heat capacity is that of only 50 cubic centimeters of water. Hence, as a result of evaporation alone, the temperature of the air column would be lowered on the average by about 6° C. Evaporation, therefore, appears to be both necessary and sufficient to produce all or nearly all the cooling of a thunderstorm.

But what is the effect of this evaporation on the density of the atmosphere? Since the molecular weight of water is 18 while the average molecular weight of air is approximately 29, it follows that the amount of evaporation above assumed would decrease the density of the atmosphere by, roughly, one part in a thousand. On the other hand, a decrease in temperature of 6° C., that would be produced by the evaporation assumed, would increase it by about one part in fifty. Hence the resultant of these two opposing effects is substantially that of the second alone; that is, a distinct increase in density, and a consequent downrush of cold air.

Doubtless, as already implied, the evaporation of thunderstorm rain, and therefore the drop in temperature and the consequent fractional gain in density, all increase with decrease of elevation. In some measure, however, this effect is counteracted by the higher temperatures of the lower layers—the higher the absolute temperature the less proportionately the change of density per degree change of temperature. But no matter how nor to what extent the details may vary, it seems quite certain that the cold rain of a thunderstorm and its evaporation together must establish a local downrush of cold air, an observed important and characteristic phenomena, really the immediate cause of the vigorous circulation, whose rational explanation has been attempted in the past few paragraphs.

As the column or sheet of cold air flows down it maintains in great measure its original horizontal velocity and, therefore, on reaching the earth rushes forward in the direction of the storm movement, underrunning and buoying up the adjacent warm air. And this condition, largely due, as explained, to condensation and evaporation, once established necessarily is self-perpetuating, so long as the general temperature gradient, humid-

ity, and wind direction are favorable. It must be remembered, however, that thunderstorm convection, rising air just in front and descending air with the rain, does not occur in a closed circuit, for the air that goes up does not return nor does the air that comes down immediately go up again, there simply is an interchange between the surface air in front of the storm and the upper air in its rear. The travel of the storm, by keeping up with the underunning cold current, just as effectually maintains the temperature contrasts essential to this open-circuit convection as does continuous heating on one side and cooling on the other maintain the temperature contrast essential to a closed circuit convection.

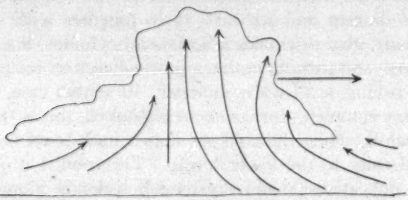
The movements of the warm air in front of the rain, the lull, the inflow, and the updraft, resemble somewhat those of a horizontal cylinder resting on the earth where the air is quiet and rolling forward with the speed of the storm. Similarly, the cold air in its descent and forward rush, together with the updraft of warm air, also resembles a horizontal cylinder, but one sliding on the earth and turning in the opposite direction from that of the forward-rolling or all-warm cylinder. In neither case, however, is the analogy complete, for, as above explained, the air that goes up remains aloft, while the cold air that comes down is kept by its greater density to the lower levels. The condition of flow persists, as do cataracts and crestclouds (clouds along mountain crests), but here, too, as in their case, the material involved is ever renewed.

The Squall Cloud.—Between the uprising sheet of warm air and the adjacent descending sheet of cold air horizontal vortices are sure to be formed in which the two currents are more or less mixed. The lower of these vortices can only be *inferred* as a necessary consequence of the opposite directions of flow of the adjacent sheets of warm and cold air, for there is nothing to render them visible. Neither can any vortices that may exist within the cloud be seen. Near the front lower edge of the cumulo-nimbus system, however, and immediately in front of the sheet of rain, or rain and hail, the rising air has so nearly reached its dew point that the somewhat lower temperature produced by the admixture of the descending cold air is sufficient to produce in it a light fog-like condensation which, of course, renders any detached vortex at this position quite visible.

This squall cloud, in which the direction of motion on top is against the storm, may be regarded as a third horizontal thunderstorm cylinder much smaller but more complete than either of the others.

Schematic Illustrations.—The above conceptions of the mechanism of a thunderstorm can, perhaps, be made a little clearer with the aid of illustrations. Fig. 105, a schematic representation of a thunderstorm in the making, gives the boundary of a large cumulus cloud from which rain has not yet begun to fall, and the stream lines of atmospheric flow into it. When the cloud is stationary and there is no surface wind the updraft obviously will be more or less symmetrical about a vertical through its centre, but when it has an appreciable velocity, as indicated in the figure, it is equally obvious that most, often nearly all, of the air entering the cloud will do so through its front under-surface. At this stage there will be no concentrated or local down current, only counter settling of the air round about, because, as previously explained, the air cataract requires strong local cooling, and this, in turn, calls for local rain.

FIG. 105.



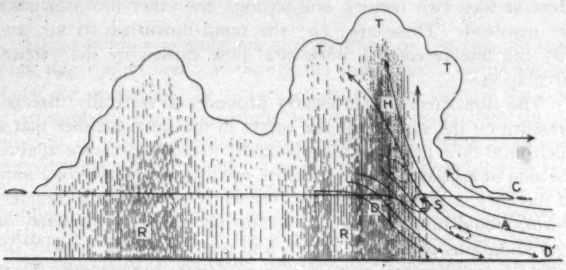
Principal air movements in the development of a cumulus cloud.

Fig. 106 schematically represents a well-developed thunderstorm in progress. The rain, often mixed with hail, cools the air through which it falls, both by conduction and evaporation, the hail also by fusion, and as the temperature gradient over a considerable area already was closely adiabatic it follows that the actual temperatures within the rain column must be lower than those of the surrounding air at corresponding levels all the way from the surface of the earth to within the cloud, that is, throughout and a little beyond the nonsaturated or evaporating region.

As soon, then, as this column or sheet of air is sufficiently cooled it flows down and forward and all the atmospheric movements peculiar to the thunderstorm are established substantially as represented.

Referring to the figure: The warm ascending air is in the region *A;* the cold descending air at *D;* the dust cloud (in dry weather) at *D'*; the squall cloud at *S;* the storm collar at *C;* the thunder heads at *T;* the hail at *H;* the primary rain, due to initial convection, at *R;* and the secondary rain at *R'*. This latter phenomenon, the secondary rain, is a thing of frequent occurrence and often is due, as indicated in the figure, to the coalescence and

FIG. 106.



Ideal cross-section of a typical thunderstorm. *A*, ascending air; *D*, descending air; *C*, storm collar; *S*, roll scud; *D'*, wind gust; *H*, hail; *T*, thunderheads; *R*, primary rain; *R'*, secondary rain.

quiet settling of drops from an abandoned portion of the cumulus in which and below which winds and convection are no longer active.

The thunderstorm is also frequently accompanied by false cirri, occasionally by scarf clouds and even, though rarely, by mamato-cumuli; but, as none of these is essential to it, all, therefore, are omitted from the above schematic illustration.

Thunderstorm Pressures.—Before the onset of a thunderstorm there usually if not always is a distinct fall in the barometer. At times this fall is extended over several hours, but whether the period be long or short the rate of fall usually is greatest at the near approach of the storm. Just as the storm breaks, however, the pressure rises very rapidly, usually from 1 to 2 millimeters, fluctuates irregularly, and finally, as the storm passes, again be-

comes rather steady but at a somewhat higher pressure than prevailed before the rain began.

The cause of these pressure changes is rather complex. The decrease in the absolute water vapor of the air as a whole, measured by the condensation, and the decrease in the temperature of the lower air—perhaps more than offset by the latent heat set free in the upper—both tend to increase the atmospheric pressure, and each contributes its share to the final result. Both these effects, however, are comparatively permanent, and while they may be mainly responsible for the increase of pressure after the storm has gone by, they probably are not the chief factors in the production of the initial and quickly produced pressure maximum. Here at least two factors, one obvious, the other inconspicuous, are involved. These are: (*a*) the rapid downrush of air, and (*b*) the interference to horizontal flow caused by the vertical circulation.

The downrush of air clearly produces a vertically directed pressure on the surface of the earth, in the same manner that a horizontal flow produces a horizontally directed pressure against the side of a house. But a pressure equal to that given by 2 mm. of mercury, a pressure increase frequently reached in a thunderstorm, would mean about 2.72 grams per square centimeter, or 27.2 kilograms per square meter, and require a wind velocity of, roughly, 50 kilometers per hour or 14 meters per second. Now, the velocity of the downrush of air in a thunderstorm is not at all accurately known, but while at times probably very considerable, the above value of 14 meters per second seems to be excessive; in fact, its average value may not be even half so great. If in reality it is not, then, since the pressure of a wind varies as the square of its velocity, it follows that less than one-fourth of the actual pressure increase can be caused in this way. Hence it would seem that there probably is at least one other pressure factor, and, indeed, such a factor obviously exists in the check to the horizontal flow caused by vertical convection.

To make this point clear: Assume two layers of air, an upper and a lower, flowing parallel to each other. Let their respective masses per unit length in the direction of their horizontal movement be M and m, and their velocities V and v. Now, if, through convection, say, the whole or any portion of the lower layer is car-

ried aloft, it must be replaced below by an equal amount of the upper air.

Let the whole of the lower layer be carried up. To produce the rainfall above assumed, 2 centimeters, this layer would have to be at least 1 kilometer deep; but no matter what its depth if it should merely change places with the upper air, there obviously could be no effect on the flow nor on the height of the barometer. Even if the different layers should mingle and assume a common velocity V'', the rate of flow would still remain unchanged, in accordance with the law of the conservation of linear momentum, and the barometer reading unaltered.

In symbols we would have the equation

$$MV + mv = (M + m)\ V'.$$

Hence neither interchange nor mingling of the two air currents, upper and lower, can change the vertical mass of the atmosphere, nor, therefore, the surface pressure. But, then, in the case of atmospheric convection there is something more than simple mingling of two air currents, and the linear momentum does not, in general, remain constant. The increased surface velocity following convection, a phenomenon very marked in the case of a thunderstorm, causes an increased frictional drag and therefore a greater or less decrease in the total flow. Suppose this amounts to the equivalent of reducing the velocity of a layer of air only 25 meters thick from V to v, and let $V = 5v$. That is, the equivalent of the one-three-hundred-and-twentieth part of the atmosphere having its flow reduced to one-fifth its former value. This would reduce the total flow of the atmosphere by about 1 part in 400, and thereby increase the barometric reading by nearly 2 millimeters.

It would seem, then, that the friction of the thunderstorm gust on the surface of the earth, through the consequent decrease in the total linear momentum of the atmosphere and, therefore, its total flow, must be an important contributing cause of the rapid and marked increase of the barometric pressure that accompanies the onset of a heavy thunderstorm.

To sum up: The chief factors contributing to the increase of the barometric pressure during a thunderstorm appear to be, possibly in the order of their magnitude: (*a*) decrease of horizontal flow, due to surface friction; (*b*) vertical wind pressure, due to

descending air; (*c*) decrease in total humidity, due to precipitation; (*d*) lower temperature, due largely to evaporation—probably more than offset by the heat of condensation.

Thunderstorm Temperatures.—Before the onset of the storm the temperature commonly is high, but it begins rapidly to fall with the first outward gust and soon drops often as much as 5° C. to 10° C., because, as already explained, this gust is a portion of the descending air cooled by the cold rain and by its evaporation. As the storm passes the temperature generally recovers somewhat, though it seldom regains its original value.

Thunderstorm Humidity.—As previously explained, heavy rain, at least up in the clouds, and therefore much humidity, and a temperature contrast sufficient to produce rapid vertical convection, are essential to the genesis of a thunderstorm. Hence during the early forenoon of a day favorable to the development of heat thunderstorms both the absolute and relative humidity are likely to be high. Just before the storm, however, when the temperature has greatly increased, though the absolute humidity still is high, the relative humidity is likely to be rather low. On the other hand, during and immediately after the storm, the relative humidity is high, owing to both evaporation and decrease of temperature, and a little later, at least, the absolute humidity, because of the removal of a large amount of moisture from the atmosphere, often, presumably, comparatively low.

"Rain-gush."—It has frequently been noted that the rainfall is greatest after heavy claps of thunder, a fact that appears to have given much comfort and great encouragement to those who maintain the efficacy of mere noise to produce precipitation—to jostle cloud particles together into raindrops. The correct explanation, however, of this phenomenon seems obvious: The violent turmoil and spasmodic movements within a large cumulus or thunderstorm cloud cause similar irregularities in the condensation and resulting number of raindrops at any given level. These in turn, as broken by the air currents, give local excess of electrification and of electric discharge or lightning flash. We have, then, starting toward the earth at the same time and from practically the same level, mass, sound, and light. The light travels with the greatest velocity, about 300,000 kilometers per second, and therefore the lightning flash is seen before the thunder is heard—its velocity being, roughly, only 330 meters per second—while the

rain, with a maximum velocity of 8 to 10 meters per second with reference to the air, reaches the earth still later. In fact, it is the excessive condensation or rain formation up in the cumulus cloud that causes the vivid lightning and the heavy thunder. According only to the order in which their several velocities cause them to reach the surface of the earth it might appear, and has often been so interpreted, that lightning, the first perceived, is the cause of thunder, which, indeed, it is, and that heavy thunder, the next in order, is the cause of excessive rain, which most certainly it is not.

Thunderstorm Velocity.—The velocity of the thunderstorm is simply the velocity of the atmosphere in which the bulk of the cumulus cloud happens to be located. Hence, as the wind at this level is faster by night than by day and faster over the ocean than over land, it follows that exactly the same relations hold for the thunderstorm, namely, that it travels faster over water than over land and faster by night than by day. The actual velocity of the thunderstorm, of course, varies greatly, but its average velocity in Europe is 30 to 50 kilometers per hour; in the United States, 50 to 65 kilometers per hour.

Hail.—Hail, consisting of lumps of roughly concentric layers of compact snow and solid ice, is a conspicuous and well-known phenomenon that occurs during the early portion of most severe extratropical thunderstorms. But in what portion of the cloud it is formed and by what process the layers of ice and snow are built up are facts that, far from being obvious, become clear only when the mechanism of the storm itself is understood.

As before, let the surface temperature be 30° C. and the absolute humidity 50 per cent., or the dew point 18° C. nearly. Under these conditions saturation will obtain, and, therefore, cloud formation begin when the surface air has risen to an elevation of approximately 1.5 kilometers. Immediately above this level the latent heat of condensation reduces the rate of temperature decrease with elevation to about half its former value, nor does this rate rapidly increase with further gain of height. Hence in mid-latitudes, where the above assumptions correspond in general to average thunderstorm conditions, it is only beyond the 4-kilometer level that freezing temperatures are reached, and where hail, therefore, can form. In the tropics and, after mid-summer, in the warmer portions of the temperate regions, where the freezing

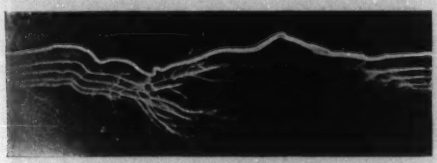
level is very high, hail seldom occurs. Generally, either it is not formed at all, owing to insufficient cloud height, or, if formed, is melted before reaching the ground from its initial great altitude.

The process by which the nucleus of the hailstone is formed and its layer upon layer of snow and ice built up seems to be as follows: Such drops of rain as the strong updraft within the cloud blows into the region of freezing temperatures quickly congeal and also gather coatings of snow and frost. After a time each incipient hailstone gets into a weaker updraft, for this is always irregular and puffy, or else tumbles to the edge of the ascending column. In either case it then falls back into the region of liquid drops where it gathers a layer of water, a portion of which is at once frozen by the low temperature of the kernel. But again it meets an upward gust, or falls back where the ascending draft is stronger, and again the cyclic journey from realm of rain to region of snow is begun; and each time—there may be several—the journey is completed a new layer of ice and fresh layer of snow are added. In general the size of the hailstones will be roughly proportional to the strength of the convection current, but since their weights vary approximately (they are not homogeneous) as the cubes of their diameters, while the supporting force of the upward air current varies, also approximately, as only the square of their diameters, it follows that a limiting size is quickly reached. It is also evident, from the fact that a strong convection current is essential to the formation of hail, that it can occur only where this convection exists; that is, in the *front* portion of a heavy to violent thunderstorm.

Some meteorologists hold that the roll scud between the ascending warm and descending cold air is the seat of hail formation, but this is a mistaken assumption. Centrifugal force would throw a solid object, like a hailstone, out of this roll probably before a single turn had been completed. Besides, and this objection is, perhaps, more obviously fatal than the one just given, the temperature of the roll scud, because of its position, the lowest of the whole storm cloud, clearly must be many degrees above the freezing point. Indeed, as the above calculation shows, temperatures low enough for the formation of hail can not often obtain at levels much less than three times that of the scud, and therefore it evidently is in the higher levels of the cumulus and not in the low scud that hail must have its genesis and make its growth.

Lightning.—About the middle of the eighteenth century Franklin and others clearly demonstrated that the lightning of a thunderstorm and the discharge of an ordinary electric machine are identical in nature, and thereby established the fact that many of the properties of the former may logically be inferred from laboratory experiments with the latter. There is, however, one important difference between the two phenomena that does not seem always to be kept in mind, namely, the distribution of the charge. In the one case, that of the laboratory experiment, the charge commonly exists almost wholly on the surface of the apparatus used, while in the other, that of the thunderstorm, it is irregularly distributed throughout the great cloud volume. Hence the two discharges, lightning and laboratory sparks, necessarily differ from each other in important details. Nevertheless, in each

FIG. 107.



Growth of an electric spark discharge. (Walter.)

case the atmosphere must be ionized before the discharge can take place freely, and this condition seems, at times at least, to establish itself progresso-spasmodically. That is, a small initial discharge, losing itself in a terminal brush, is rapidly followed by another and another, each losing itself in a manner similar to the first, until a path from pole to pole is sufficiently ionized to permit of a free electric flow and quick exhaustion of the remaining charge. Fig. 107, copied from a photograph by Walter[88], taken on a rapidly moving plate, shows how a laboratory spark spasmodically (doubtless influenced by the period of electrical oscillation) ionizes the air from either pole and thus progressively extends and finally closes the conducting path of complete discharge. There appears also to be good evidence that the lightning discharge often behaves in a manner generally similiar, though, perhaps, radically

[88] *Annalen der Physik u. Chemie, Leipzig*, 1899, **68**, 776.

different, in certain details. Thus the free period of electrical oscillation that belongs to ordinary laboratory apparatus presumably affects the process of discharge building as well as the nature of the discharge after it is fully established; while, on the other hand, if, as seems practically certain, lightning is not oscillatory, it follows that its growth into a full flash must be acquired by some process independent of a periodic surge.

Lightning, however, usually is pulsatory, as is obvious from the flicker of sheet lightning, described below, discharge after dis-

FIG. 108

Streak lightning (sequent discharges), rotating camera. (Larsen.)

charge taking place in the same direction and along the same path. Occasionally these sequent discharges extend to unequal distances, the latter especially becoming feebler and shorter, as shown in Fig. 108, thereby in their decay inversely simulating the growth or progressive development of a freely oscillating laboratory discharge. However, being pulsatory, or consisting of a group of unidirectional discharges, is an entirely different thing from being oscillatory, that is, consisting of an equally spaced series the units of which are alternately in opposite directions.

It will be convenient, in further discussing the facts known about lightning, to classify it according to its general appearance.

Streak Lightning.—When the storm is close by, the lightning discharge invariaby appears to the unaided eye as one or more sinuous lines or streaks of vivid white or pink—invariably sinuous, because electrically the atmosphere is always heterogeneous or unequally ionized and the directive force constantly changing during, and because of, the discharge itself. Often there is one main trunk with a number of branches, all occurring at the same time and apparently instantaneously, while at other times there are two or more simultaneous though locally disconnected streaks. Frequently the discharge continues flickeringly (on rare occasions even steady, like a white-hot wire) during a perceptible time—occasionally a full second.

But all these phenomena are best studied by means of the camera, and have been so studied by several persons, among whom Walter, of Hamburg; Larsen, of Chicago; and Steadworthy, of Toronto, are among the most persistent and successful. Stationary cameras, revolving cameras, stereoscopic cameras, cameras with revolving plates, and cameras with spectrographic attachments have all been used, separately and jointly, and the results have abundantly justified the time and the labor devoted to the work.

(To be continued.)

The Corrosion of Fence Wire. O. W. STOREY. (*Proceedings of the American Electrochemical Society*, October 3–6, 1917.)—The short life of present-day fence wire as compared with the older wires, some of which are still in service, has been the source of considerable trouble to the users of this product. A study of the analyses of a large number of fence wires has resulted in the conclusion that the presence of a sufficient amount of copper in steel wire determines whether that wire is resistant to corrosion. The results have without exception shown that where two steel wires have been subjected to identical corroding conditions the wire with an appreciable percentage of copper has shown the lesser corrosion. In the light of recent extensive investigations on the corrosion of iron and steel, especially copper steels, these results, covering a period of twenty to thirty years or more, are of much significance, in that they confirm the results obtained in corrosion tests covering a short period of time.

It has been definitely established that the durability of old steel fence wire is due to the presence of copper; present-day steel fence wire usually does not contain copper, and therefore corrodes rapidly. Manganese does not increase the corrosion of steel, and its absence does not decrease the corrosion. Steel fence wire containing

copper is as durable as wrought iron. The copper in the early steels and wrought irons came from the copper-bearing ores of the eastern United States and imported ores. The proportion of copper-steel made decreased with the increasing importance of the copper-free Lake Superior ores.

The life of fence wire is dependent upon the quality of both the galvanizing and the iron or steel base. Since the galvanizing is usually thin, the life of the fence depends principally upon the iron or steel base, which should be highly resistant to corrosion. Under ordinary atmospheric conditions, zinc corrodes more slowly when other conditions favor rapid corrosion of iron, and, *vice versa*, iron corrodes more slowly when conditions favor the rapid corrosion of zinc.

Railway Electrification as a Means of Saving Fuel. E. W. RICE, JR. (*General Electrical Review*, vol. 21, No. 3, p. 171, March, 1918.)—Where electricity has been substituted for steam in the operation of railroads, fully 50 per cent. increase in available capacity of existing tracks and other facilities has been demonstrated. This increased capacity has been due to a variety of causes, but largely to the increased reliability and capacity of electric locomotives under all conditions of service, thus permitting a speeding up of train schedules by some 25 per cent. under average conditions.

It is estimated that something like 150,000,000 tons of coal were consumed by the railroads in the year 1917. It is now known, from the results from such electrical operation of railroads as we already have in this country, that it would be possible to save at least two-thirds of this coal if electric locomotives were substituted for the present steam locomotives. On this basis there would be a saving of over 100,000,000 tons of coal in one year. This is an amount three times as large as the total coal exported from the United States during 1917. The carrying capacity of our steam roads is also seriously restricted by the movement of coal required for the haulage of the trains themselves. Estimates indicate that fully 10 per cent. of the total ton-mileage movement behind the engine drawbar is made up of company coal and coal cars, including in this connection the steam-engine tender and its contents. The consumption of fuel oil by railroads is also very great, 40,000,000 barrels, it is said, having been used in 1913, or nearly 15 per cent. of the total oil produced.

The possible use of water power should also be considered. According to estimates, there are not less than 25,000,000 horsepower of water power available in the United States, and if this were developed and could be used in operating our railroads, each horsepower so used would save at least 6 pounds of coal now burned under the boilers of our steam locomotives. It is true that this water power is not uniformly distributed in the districts where the railroad requirements are greatest, but the possibilities indicated by the figures are so impressive that they justify careful examination as to the extent to which water power could be so employed and the amount of coal that could be saved by its use.

PRESENTATION OF THE FRANKLIN MEDAL.
MAY 15, 1918.

AT the Stated Meeting of the Committee on Science and the Arts, held April 3, 1918, the following resolutions were adopted:

"*Resolved*, That The Franklin Medal be awarded to Guglielmo Marconi, Electrical Engineer, of Italy, in recognition of his brilliant inception and successful development of the application of magneto-electric waves to the transmission of signals and telegrams without the use of metallic conductors."

"*Resolved*, That The Franklin Medal be awarded to Thomas Corwin Mendenhall, Physicist, of Ravenna, Ohio, in recognition of his fruitful and indefatigable labors in physical research, particularly his contributions to our knowledge of physical constants and electrical standards."

CORRESPONDENCE WITH MEDALLISTS.

THE FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
Philadelphia

Signor Guglielmo Marconi,
Italian Senate,
Rome, Italy.

APRIL 8, 1918.

SIR:

I have the honour to inform you that The Franklin Institute has awarded you The Franklin Medal, founded for the recognition of those workers in physical science or technology, without regard to country, whose efforts in the opinion of the Institute have done most to advance a knowledge of physical science or its applications. The award is minuted as follows:

"That The Franklin Medal be awarded to Guglielmo Marconi, Electrical Engineer, of Italy, for his brilliant inception and successful development of the application of magneto-electric waves to the transmission of signals and telegrams without the use of metallic conductors."

The medal, and accompanying certificate are being prepared and His Excellency, Count V. Macchi de Cellere, your Government's Minister at Washington, has been requested to come to the Institute on Wednesday, May 15, to receive this medal and certificate on your behalf.

I am,
Respectfully,
(Signed) GEO. A. HOADLEY,
Acting Secretary.

GAH:W

THE FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
Philadelphia

Count V. Macchi de Cellere,
Ambassador Extraordinary and
Plenipotentiary of His Majesty
the King of Italy.

APRIL 8, 1918.

SIR:

I have the honour to inform you that The Franklin Institute has awarded to Signor Guglielmo Marconi, Italian Senate, Rome, Italy, The Franklin Medal, founded for the recognition of those workers in physical science or technology, without regard to country, whose efforts, in the opinion of the Institute, have done most to advance a knowledge of physical science or its applications.

The award is minuted as follows:

"That The Franklin Medal be awarded to Guglielmo Marconi, Electrical Engineer, of Italy, for his brilliant inception and successful development of the application of magneto-electric waves to the transmission of signals and telegrams without the use of metallic conductors."

The medal and accompanying certificate are being prepared, and I am requested, on behalf of our management, to extend to you, representing your Government, a cordial invitation to come to the Institute on Wednesday, May 15th, to receive this medal and certificate from our President, for Signor Marconi.

Two Franklin Medals have been awarded this year, one to Signor Marconi, and one to Dr. T. C. Mendenhall, Physicist, of Ravenna, Ohio, U. S. A.

An invitation will be extended to you and to Doctor Mendenhall to be the guests of honour at a dinner following the presentation ceremonies. These invitations will be formally sent you at a later date by the President of the Institute, Dr. Walton Clark.

I am,

Your Excellency's very humble servant,

(Signed) GEO. A. HOADLEY,
Acting Secretary.

GAH:W

THE FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
Philadelphia

APRIL 6, 1918.

Dr. Thomas C. Mendenhall,
Ravenna,
Ohio.

SIR:

I have the honour to inform you that The Franklin Institute has awarded you The Franklin Medal, founded for the recognition of those workers in

GUGLIELMO MARCONI

physical science or technology, without regard to country, whose efforts in the opinion of the Institute, have done most to advance a knowledge of physical science or its applications. The award is minuted as follows:

"That The Franklin Medal be awarded to Thomas C. Mendenhall, Physicist, of Ravenna, Ohio, for his fruitful and indefatigable labors in physical research, particularly his contributions to our knowledge of physical constants and electrical standards."

The medal and accompanying certificate are being prepared, and I am requested, on behalf of our management, to extend to you a cordial invitation to come to the Institute on Wednesday, May 15th, to receive this medal and certificate from our President.

I am,

Respectfully,

(Signed) GEO. A. HOADLEY,

Acting Secretary.

GAH:W

REGIA AMBASCIATE D'ITALIA

WASHINGTON, April 11, 1918.

DEAR SIR:

I have learned with the greatest pleasure by your letter of the 8th instant, that The Franklin Institute has deliberated to award to Senator Marconi The Franklin Medal in recognition of his contribution to scientific attainments, and I know that every Italian will rejoice with me and feel proud of the honor thus bestowed upon our illustrious fellow-citizen.

This expression of my appreciation I beg you to accept and to convey to the Board of The Franklin Institute.

As for the invitation extended to me to come to the Institute on Wednesday, May 15th, to receive this medal from your President on behalf of Senator Marconi, I count it as an honor and would accept it without reservations, should the duties of my office allow me to do so. In fact, I am not able to state, with such anticipation, whether at the last moment I will be free to come. But, assuring you that I would feel happy to come, I can say that, were this impossible to me, I would send a representative to act in my stead.

May I now ask you whether The Franklin Institute has already communicated directly to Senator Marconi the honor of this distinction, or whether they think it preferable to make the communication through me? If so, I would be pleased with such a task.

I am, Sir, with best regards,

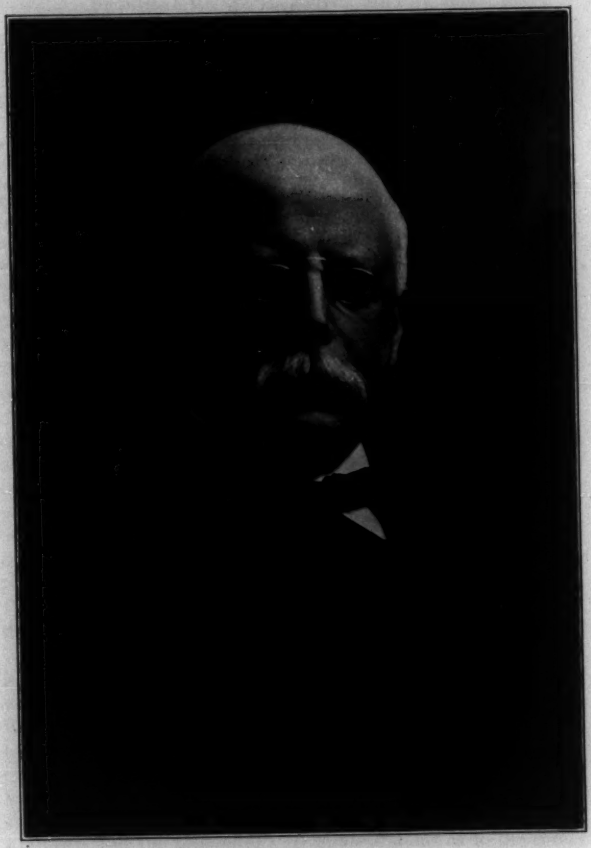
Very truly yours,

(Signed) MACCHI DE CELLERE,

Italian Ambassador.

GEORGE A. HOADLEY, ESQ.,

ACTING SECRETARY OF THE FRANKLIN INSTITUTE



THOMAS CORWIN MENDENHALL

COSMOS CLUB

WASHINGTON, D. C., April 19, 1918.

MY DEAR MR. HOADLEY:

Yours of the 6th instant has just reached me, having been forwarded from Ravenna.

I cannot tell you how surprised I am and how gratified by your notice of the action of The Franklin Institute in awarding me The Franklin Medal. It is an honor which if one may not covet, one may fully enjoy and appreciate when it comes, even though it seems beyond one's deserts. I know of nothing at present that will stand in the way of my being at the meeting of May 15th.

I am staying here until after the meeting of the National Academy of Sciences, which will be concluded two weeks from to-day. As only first-class mail is being forwarded to me, if you have an extra copy of the reprint from the Journal referring to the presentation of the medal in 1917, and will send it to me here, I will be greatly obliged.

As ever,

Yours faithfully,

(Signed) T. C. MENDENHALL.

MR. GEO. A. HOADLEY,
ACTING SECRETARY,
FRANKLIN INSTITUTE,
Philadelphia, Pa.

CABLEGRAM.

ROME, Italy, May 25, 1918.

George Hoadley,
Franklin Institute,
Philadelphia.

Please convey to Board of Franklin Institute my deep appreciation of the great honour they have conferred upon me by the award of The Franklin Medal.

GUGLIELMO MARCONI.

PROGRAMME OF MEETING, MAY 15, 1918.

Presentation of The Franklin Medal to Count V. Macchi de Cellere, on behalf of the Royal Italian Government, for Senator Guglielmo Marconi.

Presentation of The Franklin Medal to Thomas Corwin Mendenhall, Sc.D., LL.D.

Address by Doctor Mendenhall—"Some Metrological Memories."

PRESENTATION OF THE FRANKLIN MEDAL TO SENATOR GUGLIELMO MARCONI AND DOCTOR THOMAS CORWIN MENDENHALL.

IN calling the meeting to order the President of the Institute announced that the business of the meeting would be the annual presentation of the Institute's highest award, The Franklin Medal, in recognition of distinguished scientific and technical achievement, and recognized Dr. Harry F. Keller, who made the following statement relative to the work of Senator Marconi:

Mr. President: The May meeting of the Institute for the presentation of the Franklin Medal has come to be regarded as one of the red letter days on the calendar of this venerable scientific society. It is the occasion of our gathering here to do honor and pay our tribute to "those workers in physical science and technology, without regard to country, whose efforts, in the opinion of the Institute, have done most to advance our knowledge of physical science and its applications," and it is your privilege as President to present to them or to their accredited representatives the highest award within the gift of the Institute.

Heretofore the Committee on Science and the Arts has deemed it expedient to restrict its choice of medallists to men of science who were citizens of countries not actively engaged in the great European war. It is true that when these medals were presented by you in 1917 our country had committed herself to this great conflict for human rights and liberty, and more than a month had passed since our President and Congress had declared that a state of war existed between the United States and the German Empire. But the committee's recommendations as to the choice of medallists had then been made and ratified by the Institute, and there being no reason to reconsider them, but rather every reason in their favor, the presentation of the medals was proceeded with.

This year, of course, your committee approached its task under totally different conditions. The declaration of war had absolutely changed our relations towards the subject of peace and war, and so in considering the awards your committee was actuated by the desire and determination that its choice should fall upon men only whose achievements had unmistakably and materially contributed to the welfare of our Nation and that of its Allies. It is with full confidence that this aim has been realized that I now have the honor, on behalf of the Committee of Science and the Arts, to present to you our distinguished guests who are to receive from you these beautiful medals.

Of the two medallists the first belongs to a nation which, inspired by the same ideals of liberty and democracy that compelled our country to enter the great conflict, and which after several years of almost superhuman resistance against the enemy, and untold sacrifices in blood and treasure, is now our associate in arms; the other an American scientist whose career has been devoted largely to the service of the Nation, and the fruits of whose work are now seen in many departments of the scientific and engineering activities conducted by our government.

Mr. President: The Italian inventor whose achievements the Institute desires to recognize by the award of The Franklin Medal is of that brilliant type of workers who concentrate their endeavors upon a single great problem, or rather series of problems, in applied science. It is with his name that we associate an epoch-making achievement, the invention of wireless telegraphy and its successful development into a universal system of communication.

The story of how this marvellous invention came to be made and how, by successive steps, it has been perfected into a commercial and transoceanic system has been so often and well told that I may take it for granted that this audience is quite familiar with this romance of scientific achievement. In common with other great inventors the creator of the wireless art has not been spared the experience of unjust and ungenerous attacks upon his claims by envious rivals and competitors. Those who know him personally describe him as of a modest and even retiring disposition, but the results of his labors have spoken for him so eloquently that his title to the inventions in question is now beyond

the shadow of a doubt. He is universally acclaimed as an electrical engineer and inventor of the foremost rank whose achievements stand out preëminently by their international character and significance. It is to them that Italy owes the renewal of the glory in electrical science conferred upon her by the fundamental discoveries of Galvani and Volta.

Our medallist was born at Bologna April 25, 1874. On his father's side he is of purely Italian stock, but his mother was a native of Ireland. After receiving his elementary education at Leghorn he attended the University of Bologna, where he pursued scientific studies. As early as 1890 he conducted experiments to prove that electric currents can be made to pass through any medium, and that once started they will follow their course without deviation and without the aid of a conductor. A device constructed by him in 1896 for sending signals without the use of wires attracted the attention of Sir William Preece, who tested it with most encouraging results in England. The new method of sending messages was based upon the use of electric waves, the existence of which was first suggested by Clerk Maxwell and, later, experimentally verified by H. Hertz, who also succeeded in measuring their velocities and wave lengths. As I need hardly explain to you, these waves are set up by an oscillatory spark (such as that of a Leiden jar) and travel through the ether, just as light waves do, differing from the latter only in wave length. They can be received and detected at a distance by a spark gap similar to that of the transmitter. A better way, however, of detecting them was found in what is known as the *coherer*, which is a glass tube filled with metallic filings. An ordinary current will not readily pass through such particles, owing to the resistance of their loose contacts, but when the electric waves fall upon them they cohere or cling together, making the filings a conductor. Upon tapping the tube the resistance is restored. During the first ten years, when the art was still in its infancy, the method of sending and receiving wireless messages consisted essentially in the application of these principles. In 1905 our medallist devised his directive system of wireless telegraphy, and in the following year a new persistent-wave system of wireless service. Since then both the apparatus and the practice of this art have been greatly improved.

At the present time the essential features of a wireless system

are briefly these: At the transmitting station is an induction coil or transformer, the secondary of which charges a condenser to the potential at which the spark discharge passes across the gap. The frequency of the oscillations is regulated by the operator with the aid of sliding contacts. The spark then induces oscillations in the aerial-wire system (the wires of the latter being stretched (horizontally), tuned to resonance with a second sliding contact. Now the electric waves sent out by this system induce oscillations in the aerial of the receiving station, which may be thousands of miles away, and this aerial is also tuned to resonance by means of a variable capacity and an "inductance." The detector is a crystal of carborundum placed in series with a telephone receiver. The carborundum allows the intermittent current to pass in one direction only while the oscillations of a given spark continue, and then to cease until those of the next spark arrive. The operator hears a musical note corresponding to the frequency of the sparks as long as the key is depressed. Long and short notes represent the dots and dashes of ordinary telegraphy. To prevent the receiver from picking up waves of other periods a series of tuned circuits are inserted before the detector.

I shall not tire you with a detailed explanation of the apparatus and its working, as these may be found by reference to any up-to-date text on physics or electricity.*

The first successful tests of the wireless method were made in England, between Penarth and Weston, and a little later by the Italian Ministry of Marine at Spezia. In 1899 wireless communication was established across the English Channel, and in the same year the inventor came to this country, using his method in the following year to report the election. The epoch-making experiments of sending wireless messages across the Atlantic from Poldhu, Cornwall, to St. John's, New Foundland, were made in 1901, and regular wireless communication across the Atlantic was established in the following year, the formal inauguratory message being transmitted by the Governor-General of Canada to the King of England. Among other spectacular incidents in the development of the wireless system I may mention the message from the President of the United States to the

* For a very complete account see "The Principles of Electric Wave Telegraphy and Telephony," by J. A. Fleming, London, Longmans, Green and Co.

King of England, opening the wireless service from Cape Cod, Mass., to Cornwall; the voyage of the cruiser *Carlo Alberto* from England to Kronstadt Harbor in 1902, during which messages were continuously received on board of the ship from the Cornwall station; a similar trip of H. M. S. *Duncan* from Portsmouth to Gibraltar in 1903; and of the Cunard Liner *Campania,* Liverpool to New York, in 1904, when the wireless service supplied the news to the first ocean daily newspaper, the *Cunard Daily Bulletin.* In 1907 the commercial service between England and America was established. Since then the method of wireless telegraphy has been adopted in one form or another by practically all countries and made use of in every part of the globe. It may be truly said of it that it has revolutionized our means of communication, and annihilated distance on land, on the sea and in the air.

That the wizard who has made these dreams of the scientist a reality has been honored in every conceivable way goes without saying. Universities and learned societies in all parts of the world have showered upon him their degrees and medals; in 1909 he shared with the physicist Ferdinand Braun the Nobel prize in physics, and many high distinctions and orders have been conferred upon him both at home and abroad.

Although it is but a short time since Senator Guglielmo Marconi was here on a most important mission from his government, the award of The Franklin Medal to him must be made *in absentia.* But as in the case of former presentations of it to foreign scientists we are honored to-day by the presence of the official representative of the medallist's country. He has graciously consented to come here from Washington and to receive on behalf of his government, for Senator Marconi, The Franklin Medal. Mr. President, I have the honor to present to you the Italian Ambassador, his Excellency Count V. Macchi de Cellere.

The President in presenting The Franklin Medal to Count Cellere said:

Your Excellency: I have the honor, in the name of The Franklin Institute, upon recommendation of its Committee on Science and the Arts, to present to you The Franklin Medal and Diploma for transmission to Senator Marconi through the State Department of your August Sovereign, the King of Italy. This award to your distinguished countryman is in recognition of his

signal services rendered to humanity in the field of science. It is the highest honor in the gift of the Institute.

In accepting the medal for Senator Marconi Count Cellere said:

Honorable Members of the Institute: I am recipient to-day of a medal which, while destined to one of Italy's greatest sons, is an homage to Italy herself. The honor of accepting this homage in the name of Guglielmo Marconi fills me with pride, both as an Italian and a friend of the scientist whose achievements have far transcended the limits of space and time. The mysterious ways of Nature have been harnessed by him to serve the purpose of men—and the glory of the inventor has been confided to the faithful, everlasting reverence of mankind, for whom wireless telegraphy has become not only a miraculous means of communication, but also one of the greatest aids to progress and defense.

As in most cases of inventions which have been of supreme benefit to men, this one, due to the genius of a son of Italy, has meant to the inventor long years of assiduous work. It has been, in fact, the result of that combination of genius and perseverance that has given to the world the system of Galileo, the electric battery of Volta and the vision of Benjamin Franklin.

This is not a casual association of names. It is the evidence of Marconi's greatness, and of the place that Italy, thanks to her most gifted sons, has held and holds among the Nations illustrious for scientific achievements beneficial to mankind.

This medal which you, Members of this learned Institute, have conferred upon Guglielmo Marconi, will always remind him of the admiration you, men of science, and all of us, have for him.

In accepting it in his name, I thank you, gentlemen. May the friendship that unites this great Republic and Italy be as lasting as the metal in which the name of our inventor is indelibly engraved. May the glory of our two friendly Nations shine for ever, and the genius of our men hereafter be devoted entirely to the peaceful labors which alone can give mankind that high degree of civilization that has been the ideal of all past generations and must become the reality of the present one.

In introducing Doctor Mendenhall, Doctor Keller said:

Mr. President: The second medallist, as already stated, is a compatriot of ours. Advanced in years, yet vigorous as ever of body and mind, he has made the long journey from Ravenna,

Ohio, to receive from you The Franklin Medal and to tell us something from the rich store of experiences he has gathered in his long and successful scientific career. He is a typical American in that he found and created his own opportunities and indefatigably always gave the best that was in him to serve his fellows and his country. Born at Hanoverton, Ohio, October 4, 1841, he received his early education in the public schools, and acquired his scientific training without attending any prescribed courses in a college or university. When only thirty-two years of age he was called to the chair of physics and mechanics in the Ohio State University, and such was the distinction he achieved during the five years of his incumbency that, in 1878, he received and accepted a call from the Imperial University of Japan, at Tokio, where he remained until 1881. In recognition of his services as professor of physics at this institution he was subsequently decorated with the order of the Sacred Treasures by the Japanese Government, and also awarded the gold medal of the National Educational Society of Japan. Returning to the Ohio State University he continued in the chair of physics until 1884, when he was made emeritus professor. In the meantime his achievements as a scientist, engineer and educator had attracted attention in many quarters. From 1884–1886 he was professor in the United States Signal Corps, and in the latter year he was elected to the Presidency of the Rose Polytechnic Institute, which position he left in 1889 to assume the duties of Superintendent of the United States Coast and Geodetic Survey. After five years of successful and distinguished service in that capacity he was President of the Worcester Polytechnic Institute from 1894 until 1901. In addition to the many and exacting duties which these several positions imposed upon him he also gave freely of his time and energy to work as a member of many important commissions of the National Government, and it is indeed to the invaluable contributions which he made to the labors of these bodies that his fame as a scientist and engineer is largely due. Thus he served as Superintendent of the United States Bureau of Weights and Measures from 1889–1894, and during these years also he was a member of the United States Lighthouse Board, of the first Bering Sea Commission and of the United States and Great Britain Boundary Commission. In 1893 he was the delegate of the United States to the Electrical Congress. Among the

historical relics preserved and treasured in this Institute is a little document, the rough draft by the hand of our medallist of the definitions and specifications of the fundamental electrical units, the Ohm, the Ampère, and the Volt. The manuscript shows that only a few slight changes were made in the definitions as drafted by him before they were adopted and signed by the representatives of the countries that participated in the Congress. He is now the sole survivor of the distinguished group, which also included E. Mascart, v. Helmholtz, W. E. Ayrton, and H. A. Rowland.

In the brief time allotted to me I shall not attempt even to sketch the contributions our medallist has made to physical science and its applications. A mere enumeration of them would tax your patience. Suffice it to say that the principal problems that have come within the range of his labors are in the fields of terrestrial gravity, seismology, atmospheric electricity, electrical measurements and standards, and spectroscopy. For his geographical and physical investigation of Alaska he received the gold medal of the American Geographical Society, and a gold medal at the Paris Exposition of 1900 for papers upon technical education.

I cannot conclude these remarks without a brief reference to our medallist's literary productions. Besides a fine appreciation of the late Professor Henry A. Rowland they include a number of highly interesting and valuable essays, mostly in popular language, on physical and engineering subjects, recording past achievements and discussing problems of the future.

And it seems to me that it is this transition from old-time natural philosophy and engineering to twentieth century physics and its applications that has characterized the work of this eminent American man of science. That he appreciates the tribute The Franklin Institute pays to his achievements in awarding him The Franklin Medal he has shown by making the long journey to receive it from your hands, Mr. President, and by preparing an address for this occasion. I have the honor and take great pleasure in presenting to you Dr. Thomas Corwin Mendenhall.

The President then said:

Doctor Mendenhall: I have the honor, in the name of The Franklin Institute, upon recommendation of its Committee on Science and the Arts, and in recognition of your distinguished

services to humanity rendered in the field of science, to present you The Franklin Medal and Diploma, the highest honor in the gift of the Institute.

On receiving the medal, Doctor Mendenhall said:

Mr. President, Members of The Franklin Institute: It is a great honor to receive at your hands this Certificate of Admission to the ranks of the eminent men upon whom The Franklin Medal has been bestowed. I beg to express my high appreciation of this honor and to offer my grateful thanks for the distinction thus conferred.

In previous awards this medal has generally and wisely gone to those who are still vigorously aggressive in the cultivation of that department of Science or Art in which they have won distinction. It has gone to those whose intellectual faculties are still "in commission" and though distinctly given for work already accomplished it has been, in some measure, like gratitude, a pleasing "recognition of favors to come."

In the present instance, however, the Institute has generously departed from this wholesome practice, in awarding it to one whose productive activities ceased a good many years ago. During these years there has been important progress in methods, and important results have been achieved, in those somewhat restricted areas of scientific investigation, my own contributions to which have been thought worthy of recognition by the Institute. He then proceeded to read the following paper:

SOME METROLOGICAL MEMORIES.

BY

THOMAS CORWIN MENDENHALL, Sc.D., LL.D.

THOUGH generally familiar with the more recent advances in physical science, I have had no important part in them. I have chosen, therefore, to indulge in some recollections and reminiscences of work done a quarter and a third and even a half century ago; pioneer days in one of the most extensive fields in which theory and practice have joined hands.

In the progress of Science and Art the most interesting and, indeed, sometimes the most important incidents rarely find their way into the cold record of printed reports or official documents and I am assuming that the future historian will not regret finding some of them, even if trivial in themselves, permanently

recorded in the pages of the JOURNAL OF THE FRANKLIN INSTITUTE.

Reminiscence is the peculiar pastime of advancing years. Its enjoyment, however, is often marred by the seemingly unavoidable necessity for the frequent injection of the personality of the speaker. In respect to this I can only ask you to be charitable and to remember that without it reminiscence is impossible. Besides, I have the sanction of Cicero (than whom on this subject there can be no greater authority), who declared that to speak much of one's self is the privilege of old age.

We are going back, then, to a period previous to the apotheosis of science; to a period when those engaged in scientific research, even for a very small portion of their time, were but a handful compared with the thousands of to-day, to whom it is a regular occupation; when the simplest appliances for experimental investigation had to be paid for out of the generally meagre income of the experimenter, or, more frequently, constructed with his own hands.

Within the last quarter of a century science has enriched the world and the world has shown its gratitude by pouring back a fine share of its riches into the lap of science who now sits, hesitating and embarrassed, hardly knowing how to behave under such novel conditions.

Those whose memory stretches over a half century or more, recall with affection the crude apparatus of Faraday, out of which sprang the infant dynamo, and the embryo electro-magnet of Joseph Henry which, by mating with Faraday's infant, became the paternal ancestor of all the giant electro-motors and power plants of the present day. And sometimes they ask themselves if, after all, science is not just now contending with some of the obstacles and embarrassments that usually attend the progress of the *nouveau riche*.

Few of those actually engaged in research in either pure or applied science (for the word research may be legitimately used in relation to certain phases of applied science) have any notion of the difficulties under which the business was carried on thirty or forty years ago. In electricity, for example, the strongest currents available were those generated by Grove's or Bunsen's batteries and when, by combining a number of these, currents of considerable strength were obtained, insulated wire sufficiently

heavy to carry them was often not obtainable and the difficulty had to be overcome by the use of long strips of muslin, dipped in hot glue and applied with a large measure of unflagging enthusiasm.

About fifty years ago the induction coil, one of the first important results of Faraday's splendid discovery, came into useful prominence as a source of "high tension" electricity largely through the ingenuity of an American instrument maker named Ritchie, who hit upon the device of winding a number of flat, thin coils and joining them together, thus avoiding the danger of a short circuit due to the proximity of two points differing greatly in electric potential. The importance of this improvement can hardly be overestimated, for besides making possible various practical applications of the coil it greatly facilitated the study of the electric discharge *in vacuo*, with the epoch-making discoveries resulting therefrom.

As an illustration of our ignorance at that time of the potentiality of the electric spark it may be worth while to relate an incident in which one of Ritchie's Coils was used in an experiment that, as far as I know, was, and still remains unique.

I had obtained from the maker a coil of modest dimensions, but capable of giving a very heavy spark about five inches in length. In the city in which I was then living nothing so efficient had been seen and its performance created much interest among physicians and others interested in electricity. The probable physiological effects of the spark were frequently discussed, and as it happened that a criminal was soon to suffer capital punishment it was quietly proposed that permission be obtained to test the matter upon the body of the man immediately after his execution, with the possible result of restoring him to life.

The interest of the sheriff was enlisted and within a very few minutes after the victim had been pronounced dead (by a physician who was entirely ignorant of our plans) the body was lying on a laboratory table, surrounded by a small group of those who had contrived the experiment, together with a few invited guests, including the Judges of the Supreme Court of the State, who were considered useful witnesses in case the unexpected should happen.

The application of the induced current produced a very astonishing exhibition of muscular action, including violent move-

ments of the arms, legs, head and eyes, perhaps less rapid, but as great in amplitude as would have been possible had the subject been actually alive. The magnitude of the reaction fell off rapidly, however, though it had not entirely disappeared at the end of six hours from the beginning.

One of the two or three who collaborated in this experiment was Dr. Theodore G. Wormley, the famous toxicologist, who later and for many years was professor of chemistry in the Medical College of the University of Pennsylvania. It was a somewhat spectacular performance and I think it brought to us both more fame of a certain kind than we desired or had any reason to expect, for it happened that a young man had been sent to "report the hanging" by a newspaper in New York called "The Police News," and in some way he had managed to be a witness of the experiment. A subsequent issue of the journal carried a large and ghastly illustration of the scene, with alleged portraits of the principal participants.

An examination at the conclusion of the experiment revealed the fact that the neck of the criminal had not been broken by the fall, and looking at it from the standpoint of modern theory and practice, it would seem that if our treatment of the case produced any result it was rather in the way of ratifying the verdict of the jury than otherwise.

On the afternoon of July 3, 1876, I completed the installation of an electric arc light on the top of the dome of the State House at Columbus, Ohio, the first flash of which, at the precise moment when the third of July became the fourth, was the signal for an outburst of cannonading, sky-rocketing, drum beating and other forms of patriotic noise which ushered in the Second Century of the American Republic. It is said to have been the first arc light publicly shown in the State and its preparation involved much labor because at that time the only available means of generating an electric current sufficient to produce such a light was a voltaic battery of fifty or more cells which had to be transported through a narrow and winding stairway to a height of about one hundred and fifty feet. A few weeks later, in one of the halls of the Centennial Exhibition at Philadelphia I saw, for the first time, a small machine which was destined to make the repetition of such a performance forever unnecessary. It was one of the several epoch-making exhibits in that wonderful collection and

was known as the "Gramme Machine," named for its inventor who, ten years after the original discovery by Pacinotti, of Florence, had rediscovered and applied an improved method of winding the armature of a dynamo, the success of which established the fact that the child of the genius of Faraday and Henry was now big enough to be put to work. The small machine was an object of great interest to students of physical science and to the intelligent public generally. I have a very vivid recollection of being joined, during one of my frequent visits to it, by a party of two or three gentlemen, one of whom impressed me by his splendid physique and "kingly bearing," and who, indeed, turned out to be the Emperor of Brazil, whose great interest in, and knowledge of, matters relating to literature and science was well known.

From that day to this the development of the methods of producing and utilizing electricity has proceeded with a rapidity and on a scale unparalleled in the history of industrialism. The explanation is simple and is contained in a single sentence. *Electricity Was a Science Before it Became an Art.* Compared with it, the utilization of steam as a means of transferring energy has advanced at a snail's pace, because steam was an art before; through the discovery of the laws of thermo-dynamics, it became a science. In the case of electricity, especially, the science became an exact science and the transition to the art was made rapid and easy, through the application of the principles of metrology, the science and art of measurement. Within seven years after the exhibition at Philadelphia the Commissioners of the Cincinnati Exposition were offering large prizes for the best system of electric lighting—both incandescent and arc—with the announcement that the award was to be based upon actual measurement of the efficiency of the machines, with definite comparisons of the "ingo and outcome" ratio, in each of the several competing systems.

In the development of the science of electricity a beautiful system of units for electric measurement had been devised, based on the fundamental units of length, mass, and time of the metric system. It was scientifically perfect to begin with and capable of being applied in practice to any desired degree of precision. To its use, more than to any other single factor, must be attributed the phenomenally rapid development of this branch of applied science during the last two decades of the nineteenth century.

The tests made at Cincinnati are said to have been the first attempt in this country to apply these units and methods of electric measurement on a commercial scale. There were several competitors and not all of them realized that the new method of finding which was best, might bring very different results from the old-fashioned way (still persisting in certain regions of competitive industry), which consists in having the jury "take a look" at the several exhibits, to be followed by a friendly chat with the exhibitors. The Chairman of the Cincinnati Jury has not yet forgotten that it brought him, for the first time, into professional relations with the legal fraternity, because of a suit for damages amounting to fifty thousand dollars brought against him by one of the exhibitors who, by unanimous vote of the jurymen did *not* receive the first prize. However, the voltmeter, ammeter and dynamometer were triumphant, as they always will be; the verdict of the jury was not set aside and no damages were collected.

A year later came the memorable Electrical Exposition of 1884, under the auspices of The Franklin Institute, the first of its kind held in this country. There were many notable exhibits and many interesting comparative efficiency tests were made with a variety of measuring instruments, nearly all of which would be regarded with mingled curiosity and amusement by the present generation of electrical engineers. I recall one, of which we were rather proud at the time, an immense "tangent" galvanometer the coil of which consisted of a single turn, five or six feet in diameter, of a large copper rod capable of taking without shunt the heaviest current the machines could generate.

It is worth remembering that the exposition was one of four nearly contemporaneous events, each significant of the dawn of a new era in the history of pure and applied science. Running nearly parallel to each other were the Electrical Exposition in Philadelphia; a course of lectures under the patronage of the Johns Hopkins University in Baltimore, by Lord Kelvin, who there elaborated his celebrated mechanical theory of light, to which he clung with more or less tenacity to the end of his days; an International Time Conference in Washington out of which came the uniform system of time-meridians by which the world has been so much benefited; and, most important, at least in point of numbers, the annual meeting of the American Associa-

tion for the Advancement of Science in Philadelphia. The interest and importance of this meeting was greatly enhanced by the presence of the leading men of the British Association, which had adjourned its meeting, held that year in Montreal, in time for its members to reach Philadelphia for the opening of our own first session.

These several events drew together, in Philadelphia or its vicinity, a host of men interested in science. The meeting of the American Association was the largest in its history up to that time and for many years afterwards. The International Time Conference was composed of official delegates from nearly all civilized countries of the world and the remarkable series of lectures by Lord Kelvin had attracted students of advanced physics from all parts of our own country, with a good number from abroad. The managers of The Franklin Institute showed great wisdom in selecting this as the time for holding the first American Electrical Exposition.

Contemporaneous with and intimately related to it was the "Electrical Conference," lasting for a week during which Americans and many eminent English scholars joined in the discussion of important problems, many of which had arisen in the application of theory to practice in the construction of dynamos and other electric appliances. Of those who were the most active in these debates there are to-day few survivors, but I recall with pleasure the modest confidence of a young man who, while still a teacher in the Central High School of Philadelphia, had attacked the problem of the dynamo with great enthusiasm and marked success, and who was destined to become one of the founders of the greatest electrical industry in the world, to the scientific and technical success of which he continues to be, after a third of a century, one of the most important contributors.

I recall one incident in connection with that conference which I venture to relate as an interesting illustration of one of the curious phases of the mental activity, or inactivity of its president who was justly considered the most distinguished American physicist of his day. That Rowland was a "genius" no one will deny and naturally he exhibited some of the weaknesses usually associated with those who have over-specialized. At the age of twenty-four years his first contribution to the literature of science appeared in the JOURNAL OF THE FRANKLIN INSTI-

TUTE and a year later he had completed an original investigation of profound significance which at once gave him a reputation abroad. From that day he continued to cultivate his own chosen field so assiduously and intensively that he seemed to have neither time nor desire to look over the wall in order to learn what was doing or had been done in near-by areas. As president of the conference he opened its proceedings with a long and most interesting address in which he reviewed the history of electrical discovery from the time when Thales observed the attractive power of amber to the modern dynamo and Maxwell's electro-magnetic theory of light. At the proper epoch he referred, though at no great length, to the work of Franklin, enumerating briefly the principal facts for a knowledge of which the world is indebted to that great natural philosopher and ending with the remark that, "Truly, our country and this city should honor the memory of this man." The address possessed much literary merit and included an eloquent appeal for the cultivation of "pure science"—regardless of all considerations of its practical value.

But in the opinion of many of his American confreres his address was marred by a grave oversight. The work of only two Americans was referred to, while regarding that of a third, the importance of which was generally thought to rank at least next to that of Franklin, there was absolute silence! The feeling of perplexed regret by which some of us were overwhelmed was greatly intensified by the fact that Lord Kelvin, whose address followed that of Rowland, devoted a large part of his time to a eulogium of Joseph Henry and his contributions to our knowledge of electricity, comparing him, as many others have done, to Faraday. Of the men "whose lives have been patterns to the world," he said, "there is none more remarkable, perhaps, than the man of this country, Joseph Henry." He and Faraday were "patterns of scientific investigators. In some cases they went parallel and made similar scientific discoveries. Henry, indeed, preceded Faraday in the discovery of electro-magnetic induction between unmoved conductors. Henry gave the warmest welcome to all practical applications of his discoveries. He sought to make none himself. I know of no other parallel to Henry's action in this respect except that of Faraday himself."

Very soon after the adjournment of the session of the con-

ference at which these two addresses had been given I ventured to chide Rowland for the extraordinary omission of all reference to Henry, whereupon he turned upon me with that brusque abruptness with which all of his friends were familiar, and exclaimed: *"What did Joseph Henry ever do in electricity!"* It was an exclamation and not a query.

The strong personal regard that I had for Rowland from the day of my first meeting with him to the end of his life and the fact that it was my privilege to pronounce his eulogy in the hall of the institution in which he won immortal fame will acquit me of any intention, in relating this incident, to detract in the slightest degree from the admiration and esteem in which he was held by all who knew him, or to lessen the affection with which his memory is cherished by those who had the good fortune to enjoy a close acquaintance with his unique personality. There was indeed no intentional slight—strange as it may seem it was a case of absolute ignorance. The blade which cuts deep must needs be thin. But the incident should be recorded as an illuminating example of the not uncommon tendency among American men of science to under-estimate, or to be quite ignorant of the work of their own countrymen. Until within the past three or four years a foreign postmark has carried great weight.

In the meantime, although the units of electric measurement had been born perfect, as Minerva sprang from the head of Jove, their practical representations were more or less crude. The commercial use of electricity, especially as an illuminant, was growing rapidly. Contracts were being made of a most extraordinary character, often in terms of incandescent lamps or arc lamps with no specifications whatever of the quantity of light they must furnish; in other cases a candle power was guaranteed which, under the circumstances was utterly impossible, though it looked well in the contract. Under these conditions the demand for a system of practical units, backed by authority and of a high degree of precision became imperative. Fortunately the matter was still in the hands of scientific men and they were anxious to secure international uniformity. To this end conferences of an international character had already been held in Paris and agreement upon definitions of practical units of measure had been approached, gradually though somewhat reluctantly, international jealousies having interfered, especially in the selection of names

for these units. The celebration in 1893 of the four hundredth anniversary of the discovery of America furnished an opportunity for a gathering in what might be called "neutral territory," at which it was hoped a definite agreement might be reached. An International Congress of Electricians was organized, the meetings of which were largely attended by Americans and by visitors and delegates from all parts of the world.

A restricted number, about twenty-five in all, constituted the "Chamber of Delegates," the members of which had been designated as the official representatives of their respective governments.

The United States Government was represented by five delegates and it may be worth while to put upon record the method by which a selection was made, under the direction of Elisha Gray, of Chicago, who represented, in matters relating to electricity, the Commissioners of the World's Fair. A list was made of the names of all Americans who were then known to be actively and intelligently interested in electricity, including professors in universities, colleges and technical schools, persons connected with electrical industries and others, in all about one hundred and fifty, as I remember. These were requested to cast ballots for their choice of five official delegates and the five persons who received the largest number of votes were recommended to the Secretary of State by whom the appointments were formally made and who had, I believe, suggested this method of selection or at least had approved of it before it was put in operation. The Chamber of Delegates was successful in its work and, though not without much discussion, its members were practically unanimous in the adoption of definitions for the principal practical units of electric measurement. A few years ago I sent to The Franklin Institute an account of how the chamber was brought to an agreement upon the definitions of the three fundamental units, the ohm, the ampère and the volt, together with the original first draft of these definitions, signed by the members of the committee to which this matter had been referred. The Institute has published this account with a reproduction of the original draft and I need make no further reference to it here.

After the work of the International Congress of 1893, the next important step was to secure the legalization of the units there defined by enactment of law or otherwise when possible. In

the matter of weights and measures as regards both legislation and practice we are, in this country, still in a condition of chaos, and it is a great tribute to our business capacity that we have managed to get on under such a handicap for nearly a hundred and fifty years. The Constitution puts upon Congress the responsibility of "fixing standards," but in spite of the strong and frequently repeated urging of our first three presidents, Washington, Jefferson, and Adams, Congress has never had the courage to discharge its duty in this respect. As far as the business of the government itself is concerned (except in the matter of coinage of money and up to the time of the legislative action of which I shall shortly speak) the Secretary of the Treasury was the final arbiter in all matters relating to standards, a function now belonging to the Secretary of Commerce, the Bureau of Weights and Measures having been transferred to that Department upon its organization by Congress.

During the period under consideration this Bureau was, as it has been for nearly a hundred years, a Division of the United States Coast and Geodetic Survey and as ex-officio Superintendent of Weights and Measures it was my duty to try to secure formal adoption of the new units; first, by securing the approval of the Secretary of the Treasury, in virtue of which they would become the legal measures for all government transactions concerning electricity; and second, through the passage of a general law which would impose them upon the country as a whole. That the latter should be accomplished as soon as possible was evident from the following considerations. In matters relating to units of measure in ordinary use, pounds, yards, gallons, bushels, etc., we were cursed and still are by the legalization of many different standards in different parts of the country. What Congress has failed to do, individual states have been compelled to do, with the natural result of a lack of uniformity as we pass from one state to another. We are now dealing with a new commodity that could not be measured in pounds or gallons, or yards, and in many places purely arbitrary and unscientific units had already come into use and there was constant danger that some state legislature might undertake to distinguish itself by legalizing them. When we reflect that in recent years there has been an attempt by at least one state legislative body to fix by law the ratio of the circumference of a circle to its diameter, the

danger does not seem so remote. In securing the approval of the Secretary of the Treasury to a bulletin from the Office of Weights and Measures announcing the adoption of the new units by the government itself I anticipated little difficulty. My time of trial in that respect had been about a year earlier when, with his aid I had accomplished, on paper, in about ten minutes, what many of us had been struggling for more than a quarter of a century to bring about, in practice, namely, putting the country in the matter of weights and measures upon a metric basis and discarding the yard and pound as fundamental standards. But the paper victory was, in fact, of great value. Even as a matter of strategy it was a position worth taking, for it was an important turn in a revolution that will never go backwards. Previous to the fifth of April, 1893, our real standards of length and mass had been the English yard and pound of which we had imperfect copies. On that day I sat for a short time with the Secretary of the Treasury, having in my hand a typewritten copy of what was to be Bulletin No. 26, of the Office of Weights and Measures, in which I had embodied a brief statement of existing metrological conditions along with the announcement that in the future the standard of length in the United States Government would be the international meter and the standard of mass would be the international kilogramme, very beautiful and perfect copies of both of which we had received from the International Bureau of Weights and Measures at Paris; that hereafter a yard would be defined as a certain fraction of a meter and a pound as a certain fraction of a kilogramme. I had approached the interview with no little anxiety, the forecasting of the attitude of a cabinet minister under any given conditions having never yet reached the dignity of an exact science. However, the secretary, John G. Carlisle, of Kentucky, showed an intelligent interest in my discourse and without making any objections affixed his signature to the paper. After the lapse of a quarter of a century during which there has been no going back on this action, I may be allowed to confess that I have never had greater pleasure in seeing a government official sign his name than on that occasion.

The principal obstacle to the passage of an Act of Congress legalizing the electric standards for the whole country was the well-known inertia of legislation which is sometimes so great that measures unanimously approved die because of sheer in-

ability to get themselves out of the committee room. In the preparation of the Electric Units Bill I had the highly valued assistance of the Chairman of the House Committee on Weights, Measures and Coinage, Mr. William A. Stone, of Pennsylvania, who was most actively and intelligently interested in all matters relating to weights and measures. Under his wise and experienced guidance the measure passed both Houses of Congress, and on July 12, 1894, became a law on receiving the signature of the President. The pen with which this memorable act was completed is now in the possession of The Frnaklin Institute along with the original draft of the definitions of the fundamental units already mentioned.

It is perhaps worth while to remark that in accomplishing these results, the importance of which will continue to increase as the years pass, we were greatly aided by the fact that little significance was attached to the measure by the general public at that time. Ohms, ampères and volts had a far-away sound to most members of the fifty-third Congress who did not suspect that they were soon to be used in measuring an annual output valued at hundreds of millions of dollars.

In somewhat remote connection with the Electrical Congress of 1893 there occurred an incident so purely personal in its character that I should never think of relating it in public were it not that the deplorable events of the past three or four years have given it a new significance, such as, in my opinion will justify its preservation in print.

The Honorary Chairman of the Congress, by unanimous choice of the committee of arrangements, was Baron von Helmholtz, of Berlin, almost equally famous as a mathematician, physicist and physiologist, and without doubt the most distinguished natural philosopher Germany has produced. He was accompanied on his visit to this country by his wife, and after the close of the Congress the two spent some days in Washington affording some of us an opportunity of knowing them more intimately. On one occasion, while in conversation with Baroness von Helmholtz, she said to me: "Do you know how my husband happened to come to this country?" I replied that I knew only that we had invited him and were very glad that he was able to accept, for at first he was disposed to decline on account of his advanced years. "Well," said she, "it happened in this way.

The Kaiser sent for him. 'Von Helmholtz,' he said, 'we are sending to this World's Fair in America the finest products of our factories, our mills, our fields and our mines; some of our choicest works of art will be there, but above all of these Germany is most proud of the men she produces. You are the best we have and you must go to represent us.'"

The Baroness told me this with evident and justifiable pride; and then, after a short pause which seemed reflective in its nature she turned to me and with upraised finger and flashing eye, added: "But do you know that when my husband walks the streets of Berlin he is pressed off the sidewalk by any petty lieutenant in the army whom he may happen to meet?" I confess to finding a ray of hope in this incident.

Metrology is fortunate in that it is both a science and an art, its two phases being mutually inspiring. So clearly is it related to the progress of civilization that the degree of precision which a nation demands in its weighing and measuring is an almost infallible index of the enlightenment of its people. Measured by this standard our own country ranks among the first. The establishment a few years ago of a National Bureau of Standards was the one thing needed to give it, in this respect, a place among those nations which, at least until recently, we were accustomed to call the "most enlightened." During the few years of its existence it has accomplished extraordinary results in the dissemination of both the principles and practice of accurate mensuration among the people generally.

In Michelson's Wave-length Standard we have found a natural unit of length, corresponding in the degree of accuracy with which it may be determined to our natural unit of time and perhaps more permanent than that, a contribution to the science of metrology of great value. To the art of metrology we have also contributed much.

If time permitted I should like to speak at length of some of the wonderful accomplishments of this art-science, but I must content myself with a brief reference to some of the results that are easily reached in the case of the fundamental magnitudes, time, length and mass.

For time, Nature has given us a unit of measure, the period of the revolution of the earth on its axis, which, as far as we know, has not varied during the historic period, though it is well

known that forces are at work which must have affected it and which will continue to affect it, until sometime in the remote future the change can be detected. With such an excellent and comparatively stable unit our derived measures and measuring devices are very satisfactory. Any given interval of time may be determined with almost any desirable degree of accuracy. Astronomers tell us the length of the mean solar year within a tenth of a second, which means that the result is correct within one part in three hundred millions. On the other hand, a small interval of time, even smaller than a second, may itself be determined within one part in a million or perhaps more accurately.

The length of a bar approximately one meter long can be ascertained in terms of our unit of length with a probable error of one part in thirty millions. Of all measurements depending upon arbitrarily chosen, non-celestial units the process of weighing is by far the most accurate. A mass of matter can be compared with our standard with a degree of precision measured by a probable error of one part in three hundred millions. The balance almost rivals the multiplication table in accuracy.

So highly developed is the art of metrology that in the application of laboratory methods to practical measurements, an almost equally high standard may be maintained. The United States Coast and Geodetic Survey has for many years required in all of its more important geodetic operations a degree of precision ranking very well within the results given above, especially when the great difficulty of out-door work is considered. Base lines six or eight miles in length are measured with a probable error of one part in three millions; that is, for the whole distance the error might be as much as one-eighth of an inch. The probable error of a field determination of the value of gravity is about one part in a million. The latitude of a station is determined with a probable error of not more than one-tenth of a second, which means that the position of a point in latitude is known to within a distance of about ten feet. In longitude the same order of accuracy is reached. The probable error of a single observed direction, as in the system of primary triangulation, is sometimes less than a quarter of a second, a quantity that may be visualized by the statement that one of the great guns with which, unhappily, we are becoming familiar, if pointed with the same degree of accuracy and assuming no other cause

for deviation, should deliver its shot at a distance of seventy-five miles within a circle of about five inches in radius.

Metrology, itself both a science and an art, is the mother of science and of art; certainly of all the exact sciences and of nearly all of the arts. Without it there could be no astronomy, no physics, no chemistry, no engineering, no architecture. There could be no trade, no commerce; take it away and the whole fabric of civilization would disappear. It is a great pleasure and privilege to sound its praises in the hall of an institution that has contributed so largely to its development.

Electric Arc-welding in Railroad Shops. ANON. (*Railway Age*, vol. 64, No. 14, p. 898, April 5, 1918.)—Welding is now demanding more attention than any other branch of the electrical field. It is used effectively on steel and cast-iron and experiments on other metals, such as bell metal, have proved that the use of arc-welding is not limited to iron and steel. Its use on railroads is being steadily extended.

In the locomotive repair shop arc-welding is used for making almost every kind of repair. Broken locomotive frames are repaired quickly and with little cost; cracked and broken cylinders are repaired without removing the cylinders from the locomotives; broken fireboxes are repaired; flue sheets are welded into boilers without the use of rivets, and it has become quite common practice to weld the flues to the sheet at the firebox end of the boilers. A cross-head guide was worn down until practically useless, and then built up by the metal electrode process. After it has been machined it will be put into service again. This practice is not limited to flat surfaces only, but applies equally well to cylindrical forms which are in need of bushing. Extensive tests have shown that it is possible to make a weld stronger than the original metal. This statement is true for cast-iron as well as for steel.

The savings made possible by the use of electric arc-welding are apparent to anyone who has seen it in use. The superintendent of a large Eastern shop recently said that by the use of improved shop methods and electric arc-welding they had been able to maintain total operating costs constant in spite of the tremendous increases in cost of labor and material.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

EFFECT OF THE SIZE OF GROG IN FIRE-CLAY BODIES.[1]

By F. A. Kirkpatrick, Assistant Physicist.

In those industries which use crushed and ground raw materials in forming bodies by means of a cementing action of some sort, the size of the particles plays a most important rôle. This may be in many instances the main factor controlling the desired properties in the body. This is nowhere more true than in the ceramic industries, where in compounding bodies the manufacturer is compelled to use materials from the size of pebbles down to the dispersoid and emulsoid (colloidal) states. All present evidence tends to show that in disperse systems the size of particles which we cannot now measure has as much effect upon the properties of certain materials as have the larger sizes which we are able to measure. In no other way is it possible to account for the great differences in cementing power exhibited by fine-grained bond clays and other fine-grained materials. In practice the cementing action takes place at temperatures from zero to that of the electric arc, depending upon the nature of the process, and is due to many different chemical reactions. In the present work the bonding power of mixtures composed of raw clay and calcined clay is determined after these bodies have been heated to 110° C. and to 1250° and 1300° C.

The control of the strength of raw fire-clay bodies is a difficult matter into which enter a number of factors. Those which are directly connected with the size of grog are size of the body, cracks formed in drying, density or porosity, and size of grain of the clay.

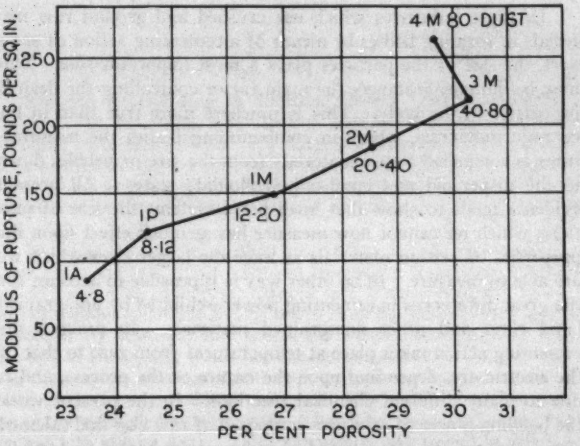
If the grog is too large for the size of the body used, the conditions of bond are different than for smaller grog. Small cracks may or may not form in drying. Hence, it is possible that the

* Communicated by the Director.

[1] Technologic Papers of the Bureau of Standards, No. 104.

curve of Fig. 4 would be much different for test pieces of larger dimensions. The general rule in ceramic bodies is that strength increases with decrease of porosity. This factor may be overruled by others, as the results in Fig. 4 show. In series 4, however, the rule holds. As shown in connection with series 5 and 7 the size of grain of the bond clay also affects the strength of the body. In regard to the grog, aside from other considerations, the following condition is necessary for highest strength: The mix-

FIG. 4.



ture of sizes must be such that the smaller particles fill the voids between the larger, giving maximum density. The proper proportions may be predicted qualitatively from the ratio of sizes, but can be determined accurately only by experiment.

The strongest raw bodies were those in series 4 and 7. In series 4 these had the following limits of grog composition: 25 to 66⅔ per cent., 20 to 40 grog; 0 to 25 per cent., 40 to 80 grog, and 33⅓ to 66⅔ per cent., 80 to dust grog. In series 7 the strongest bodies were those containing the greatest percentage of 80 to dust grog.

The control of strength in the burned state proved to be not at

all difficult. For all bodies used the modulus of rupture was found to increase with increase of surface factor. The relation was represented by means of straight lines except that the lower portions of some of the curves representing large-sized grog were of parabolic form. The rate of increase of strength increased with the temperature of burning, due to the more rapid rate of solution of the finer particles at the higher temperatures. The porosity at cone 12 varied much the same as in the raw bodies and had little relation to strength. The strongest bodies were those in series 4 (20 to dust grog). No relation was found between strength in the raw state and in the burned state.

In the quenching tests from 600° C. and 1000° C. mixtures of the larger sizes of grog gave the more resistant bodies.

Development of the Pneumatic Tire. Anon. (*Scientific American*, vol. 118, No. 1, p. 10, January 5, 1918.)—The pneumatic tire has been generally regarded as a device of comparatively recent origin, but as early as 1846 it was successfully tried in London. In that year the frequenters of one of the parks of the British metropolis were treated to a genuine surprise by the appearance of a brougham equipped with extraordinary tires upon which the vehicle seemed to float along the road without jar or noise. They consisted essentially of an inner and outer band of leather, about a foot wide, enclosing between them the pneumatic tube by being brought together around the tube and permanently riveted along one edge and laced along the other edge. These pioneer pneumatic tires were known as "patent aerial wheels" and were the invention of Robert William Thomson, who patented the idea in 1846. Such large tires naturally gave the vehicle an unusual and unattractive appearance, and that may explain why, in spite of excellent performance in service and in tests, in that day when the question of appearance rather than utility received paramount consideration, the proved merits of the pneumatic tire were ignored and remained unrecognized until the introduction of velocipedes and bicycles in the 70's, but mostly in the early 80's, created new interest in the tire question. In substance the patents of that period related mainly to the general design and means of attachment of solid tires, and only one patent applied for in 1884 varies from the solid-tire idea in calling for a hollow-centre tire, or porous or spongy centre, in order to obtain greater resiliency.

The real beginning of the present-day pneumatic tire starts from the year 1888 with the single-tube tire held to the rim by wrappings of tape, invented by John Boyd Dunlop, a veterinary surgeon of

Belfast, Ireland, from which eventually grew a tremendous tire manufacturing business. Rapid progress was made, and in 1895 the Dunlop interests took the form of a $25,000,000 corporation, which for a while enjoyed a practical monopoly in Great Britain and made extensive sales elsewhere. Casting about for a better means of attaching their products, the Dunlop interests came across the patent of Charles Kingston Welsh, who had been granted an English patent in 1890. In addition to an air tube, Welsh's patent provided a cover of rubber and canvas having thickened edges, through each of which ran a reinforcing wire. The last-mentioned member formed a complete circle, and when applied to the rim it engaged the grooves especially shaped for the purpose. An American patent covering the identical idea was granted to A. T. Brown and G. F. Stillman, of Buffalo, N. Y., in 1892. This was acquired by the Dunlop interests for the sum of $100,000 and served as the foundation for the extensive Dunlop business in America at a later period.

In 1890 the Dunlop interests acquired the patent rights to the invention of William Easkine Bartlett, covering the clincher principle of tire attachment. Instead of wires for holding the tire in place, the cover was made with beaded edges which engaged the incurving flange of the rim. The tire was first inserted in the rim and then inflated so that the beaded edges became tightly wedged in place. It is said that $100,000,000 was paid for the patent rights, the vendors besides retaining certain shop rights. A similar clincher tire was developed and patented in the United States by Thomas B. Jeffrey in 1891 and 1892. For bicycle use the single-tube tire became the most popular. However, with the advent of the automobile the clincher tire took first place in practically every country where motor vehicles appeared.

Automobile development has been consistently accompanied by tire development. Of late, tire fabric has been somewhat altered, for it is found that heavy cords give better service than the woven fabrics usually employed. It was a long step in the manufacture of commercial tires from the 1½-inch bicycle tire to the 4½-inch automobile tire in a year or two. The limit in size seemed to be governed by the resistance of the close-woven fabric to the deformations incident to the manufacture of the larger sizes. Progress in tire design during the past two years has shown that this limit no longer holds if laminated cords without cross weave are used instead of woven fabric. These cords take the strain without undue fatigue, and now we see our way clear to make 12-inch tires just as readily as 4½-inch tires. At this time pneumatic tires are being employed on very heavy motor trucks, which, with their 12-inch tires, are maintaining regular transportation service between distant cities and in this manner relieving the war-time congestion of our railroads.

No better proof of the magnitude of the pneumatic tire industry can be offered than the fact that in 1917 some 18,000,000 tires were sold, which, at an average cost of $25, totalled $450,000,000.

NOTES FROM THE NELA RESEARCH LABORATORY.*

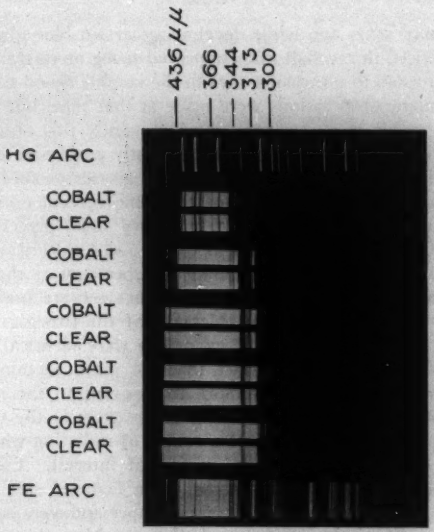
ULTRAVIOLET TRANSMISSION OF CLEAR AND COBALT-BLUE GLASSES.

By M. Luckiesh.

SEVERAL years ago while developing certain blue glasses the writer noticed that cobalt glass appeared to be more transparent than clear glass to ultraviolet rays in the neighborhood of 300$\mu\mu$. No systematic observations were made at that time, but in spectrophotographic work carried out subsequently this observation was verified on various occasions. Recently conclusive evidence was obtained by means of clear and blue glasses (soda-lead) of (1) equal thicknesses and (2) of the same content except that the blue glass contained in addition a very small percentage of cobalt. Clear and blue specimens of the same thicknesses were compared in pairs by photographing the spectrum of the quartz mercury arc through each. Various thicknesses were studied, but for each comparison pair, the thickness of the two glasses was the same, and the photographic exposures were identical. These experiments established the fact that the glass containing the slight amount of cobalt was more transparent to the mercury lines near 300$\mu\mu$. However, owing to the gaps in the mercury spectrum, it was desirable to use a source of radiation which was richer in energy in the spectral region of interest. Clear and blue glasses of the same thicknesses were fastened side by side by means of a narrow strip of gummed paper and were so placed across the slit of the spectrograph that the blue glass covered half the length of the slit and the clear glass the other half. Through such pairs, spectra of skylight, of the bare iron arc, and of the quartz mercury arc were obtained. This arrangement eliminated any photographic errors and care was taken to have the slit illuminated uniformly throughout its length. Various densities of the blue glass were studied in testing pairs of different thicknesses. An idea of the density of the blue glass may be obtained from the observation that at the lightest densities it

* Communicated by the Director.

transmitted about one-third of the light from a tungsten filament (Mazda C) lamp. None of the specimens would be considered dense. The difference in the spectral transmissions is, of course, more conspicuous on the original negatives than on prints or half-tones because such photographs suffer materially in printing and half-tone processes. In the accompanying illustration the spectrum at the top is that of a bare quartz mercury arc obtained



with a quartz spectrograph. In the five pairs following, the upper spectrum in each case is that of the iron arc through cobalt-blue glass and the lower is the spectrum of the same illuminant through clear glass of the same content except for the addition of cobalt in the blue glass. Each pair represents results from the two kinds of glasses of equal thickness under identical photographic and illumination conditions. In fact, the two spectra of a pair were made simultaneously. The spectrum at the bottom is that of the bare iron arc.

Since this observation was first made it has appeared to the writer that certain blue glasses which have been developed chiefly for scientific purposes owed a portion of their satisfactoriness to this augmented transparency in the region of 300$\mu\mu$. From a purely theoretical standpoint this result is not particularly startling; nevertheless, from a practical viewpoint, it has been somewhat surprising. No quantitative measurements of the difference have been made, but it is obvious that the difference is appreciable. For this reason the addition of a slight amount of cobalt may be of practical advantage in some cases.

CLEVELAND, OHIO, June, 1918.

Hydraulic Rams for City Water Supply. T. H. CARVER. (*Engineering News-Record*, vol. 80, No. 21, p. 1000, May 23, 1918).—The Maple Leaf pumping station of the City of Seattle represents a new departure in the common practice for the construction of water-power pumping stations. This plant should be of interest to engineers, since it is believed that it develops perhaps the highest known combined power and pumping efficiency of any existing plant of like size or larger, and in addition shows unusual economy in installation and operating costs. It consists of two 12-inch Hill hydraulic rams operating under a 50-foot power head and against a pumping lift of 140 feet from a point above the rams to the full tank level. Performance tests showed a range of capacity of water pumped from 720,000 to 1,300,000 gallons per day with corresponding efficiencies of from 90.8 per cent. to about 85 per cent. The normal daily operating capacity is something over 1,000,000 gallons of water pumped.

Eventually the plant capacity will take care of some 28,000 population and furnish ample fire protection for the 790 acres involved. The rams are simple in operation. They are started with an auxiliary starting valve, and adjustment for capacity is made by turning an adjusting screw up or down. All mechanical movement is limited to simple valve shifts of small motion, and as all moving parts are inside the machines, in water, no oil is used or required. The rams operate continuously in the locked gate house and inspection is made only once or twice a week by the regular water department employee. Maintenance and renewals, limited largely to new valve seats, are estimated to cost not to exceed $10 per year per ram. As wear is exceedingly light, the rams maintain their full efficiency for long periods.

Ball Bearings for Electric Motors. A. H. MACCAFFRAY. (*American Machinist*, vol. 48, No. 15, p. 613, April 11, 1918.)—Compactness is required under conditions usually found in general industrial applications for both alternating-current and direct-current motors. To obtain this, motor manufacturers develop, or try to develop, the greatest possible horsepower from given frame sizes. The reduced size per horsepower capacity gives greater output for acquired motor space.

Babbitt bearings are usually designed with an over-all length of two and one-half or three times the diameter of the shaft which they are called upon to support. Contrasted with this, the length of the ball bearing proper on the shaft usually is not over one-third of the shaft diameter. Through ball bearings the over-all length of motors can be reduced from 10 to 30 per cent. To be sure, on motors in which the shaft is subject only to torque, bearing friction is a small percentage of the motor loss. This condition obtains for the most part in direct-coupled units, such as pumps, motor-generator sets, etc. These bearings carry little, if anything, above the weight of the rotor, and the pressure between the shaft and the bearing is slight and friction is at a minimum. Wherever such motors are running at high speeds even slightly unbalanced forces will produce considerable bearing loads that tend to increase friction and wear. Where the shaft is subjected to bending strains, bearing friction becomes an important consideration. This is the case of belt- or gear-driven motors. The bearings are subject to severe binding, pounding, or shock, especially at the drive end. Ball bearings in these instances show a very decided saving over plain bearings, especially if the self-aligning type, which compensates for shaft deflection or misalignment, is used. Power saving in ball bearings is entirely due to the fact that their friction is practically constant over a wide range of loads and because rolling is substituted for the rubbing action of plain bearings. As ball bearings consume no more energy at start than they do at speed, acceleration is rapid and the starting torque improved several hundred per cent. Belt slippage is eliminated and it becomes possible to use smaller belts and pulleys.

To illustrate how a ball-bearing motor will run more satisfactorily under conditions of neglect than a plain-bearing motor, observe how two motors operating side by side, one on ball bearings, the other on plain bearings, will act. The plain-bearing motor, as soon as the oil has become exhausted, will heat up and the bearings will run to destruction, with the likehood of the armature being stripped when bearing wear is sufficient to let the rotor rub on the pole pieces. On the other hand, the ball-bearing motor, because of the lubricant being held in the sealed chambers, and its lubricating properties not being exhausted as quickly as in plain bearings, will run for an indefinite period without showing any signs of heating or bearing wear. A motor of this kind requires oiling only three or four times a year to insure perfect bearing operation.

NOTES FROM THE RESEARCH LABORATORY, EASTMAN KODAK COMPANY.*

THE EFFECT OF THE IRON CONTENT OF AMMONIUM PERSULPHATE ON ITS PHOTOGRAPHIC REDUCING POWER.[1]

By S. E. Sheppard, D.Sc.

It is well known that different commercial preparations of ammonium persulphate show wide variations in their behavior in attacking the density of photographic negatives. The typical peculiarity of persulphate, namely, its selective action on the higher densities, has received many explanations. In this direction the most important contributions to the chemical mechanism of persulphate reduction are the following:

H. Marshall (*Trans. Edin. Phot. Soc.*, 1900, **23**, 168) showed that the action of persulphate on metallic silver is what is termed auto-catalytic, *i.e.*, it is accelerated by the reaction-product, the dissolved silver sulphate. A chemical explanation of this is the formation of an intermediate *silver persulphate*, which is a stronger oxidizer than the ammonium or potassium persulphate; the action in question may be considered to take place in stages represented by the following chemical equations:

1. $2\,Ag + (NH_4)_2S_2O_8 = Ag_2SO_4 + (NH_4)_2SO_4$ Very slow

This reaction, the formation of silver ions by the direct attack of persulphate on metallic silver, is very slow.

2. $Ag_2SO_4 + (NH_4)_2S_2O_8 = Ag_2S_2O_8 + (NH_4)SO_4$ Very rapid

This reaction, a purely ionic interchange, is very rapid.

3. $2\,Ag + Ag_2S_2O_8 = 2\,Ag_2SO_4$ Rapid

Finally, the attack of silver persulphate upon metallic silver, is also a rapid reaction.

* Communicated by the Director.

[1] Communication No. 60 from the Research Laboratory of the Eastman Kodak Company.

In the absence of silver ions (or other catalyst) the reaction is slow at first, then becomes rapidly accelerated.

By adding silver ions, *e.g.*, silver nitrate or, better, silver sulphate, to a sluggish persulphate solution, the initial retardation is overcome.

It was suggested that this auto-catalytic action of the dissolved silver afforded an explanation of the superproportional action. This, however, is not the case. Unless other factors intervene, the rate of reaction would still be proportional to the density, so that the undoubted acceleration by dissolved silver is not a complete explanation.

Different observers had noted that the water used in making up a persulphate solution affected the action. This was taken up by E. Stenger and H. Heller (*Zeit. Wissent. Phot.*, 9, 73, 1910). They found that, depending upon the amount of chlorides in tap-water, the type of reduction changes from the superproportional to the subtractive. They attribute this chloride poisoning to the formation of relatively insoluble silver chloride, which lowers the concentration of the catalytic silver ions and protects the silver grain from further attack.

It can, however, be shown that even with distilled water free from chlorides the anomaly persists. Schuller (*Phot. Rundsch.*, 1912, p. 236) brought forward more definitely a suggestion often tentatively advanced, namely, differential diffusion. Schuller adopts the silver persulphate or catalytic theory, but considers that superproportional action on the higher densities is much less determined by poisoning in the lower densities than by a restriction on diffusion in the higher densities.

As against these explanations, Lüppo-Cramer (*Colloid Chem. und Photographie*) advanced a colloid chemical theory. He considers that the composition of the developed and fixed negative image is not pure metallic silver, but remains an adsorption compound of silver and photohalide. The amount of photohalide and, *a fortiori*, of normal silver halide, is very small, but it is supposed to be greater in proportion as the exposure is less. There is then more photohalide in the lower densities than the higher. This he suggests is due to a more energetic attack of the developer on the grains of the higher, more exposed densities.

Lüppo-Cramer regards this as an explanation of the peculiarities of photographic reducers. Reducers with a silver halide

solvent present in solution with the oxidizing agents—as in Farmer's reducer—will attack the lower densities more strongly than the upper, and we get subtractive action. On the other hand, in reducers like persulphate, or in chromic acid poisoned with soluble chloride, the lower densities having more halide are better protected from attack.

It is to be noted that the facts and explanations brought forward still fail to account for certain minor characteristics of persulphate reducers. One of these is the already mentioned variation, particularly of initial activity, between different preparations having the same persulphate content. Another is the action of acid and alkali on this initial activity.

Lüppo-Cramer, in support of his photohalide theory, instanced the equalizing action of sulphocyanide (a silver halide solvent) upon ammonium persulphate. This combination was first recommended by Bayley and Puddy (*Phot. News*, 1900, p. 174) as giving a proportionate action.

In the course of some experience intended to test Lüppo-Cramer's views on the negative image, Bayley and Puddy's combination was tried. It cannot be said that it is very satisfactory either chemically or photographically. In the first place, like other complex cyanides, sulphocyanide is attacked by persulphate with the liberation of hydrocyanic acid. In the second, measurements with sensitometer strips show that there is no certainty of proportionate action.

In trying the combination with different persulphate preparations it was observed that they all reacted, more or less vigorously, for *iron*. Further, it was noted that the preparation showing the least initial activity, or the greatest initial sluggishness, gave the weakest reaction for iron, while the most vigorous preparation, one, in fact, very difficult to control, gave the strongest reaction.

The preparations were therefore analyzed for iron, with the results given later.

On adding ferric sulphate to the solution weakest in iron till it was approximately equal in this respect to the strongest, the initial activities were made practically equal. Quantities of iron in excess of this relatively small amount gave reducers of uncontrollable character, the action becoming very irregular and localized.

The small variable amount of iron salt present as an impurity in ammonium persulphate is thus the cause of its variability, and iron, however introduced, is a powerful catalyzer of the action of persulphate on metallic silver. The chemical reactions involved show that we have here a case where a side-reaction supplies more rapidly the auto-catalyst for the main reaction, while the main reagent re-energizes the side reagent or catalyst. Iron, as a ferric salt, *e.g., ferric sulphate,* rapidly attacks metallic silver, forming silver sulphate and ferrous sulphate:

4. $Fe_2(SO_4)_3 + 2\ Ag = Ag_2SO_4 + 2\ FeSO_4$

and the *ferrous sulphate* is rapidly reoxidized to *ferric* by persulphate.

5. $2\ FeSO_4 + (NH_4)_2S_2O_8 + Fe_2(SO_4)_3 + (NH_4)_2SO_4$

so that the concentration of ferric iron tends to be kept up to the initial level. Further, the silver salt (or silver ions) supplied by reaction (4) give the necessary acceleration to the persulphate reaction proper, these reactions being those already noted in equations 1, 2 and 3.

The function of the iron then is somewhat like that of a pilot flame. By immediately furnishing silver ions, through reaction (4), it abbreviates the initial retardation and gets the rapid following reactions in full swing without delay. At the same time, the catalytic action of iron explains in part the effect of acidity and alkalinity on the persulphate reducer. So long as an acid reaction obtains, the iron will be in the active ionized condition. If the reaction becomes alkaline, the iron will be progressively removed by hydrolysis, being converted to ferric hydroxide, which, moreover, is strongly absorbed by gelatine, with a tanning effect and probable reduction in diffusion velocity.

It is necessary to note at this point that this iron catalysis does not stand in any contradiction to the other aspects of persulphate reduction. In particular, experiments with iron catalyzed persulphate strongly support the theory that it is a superposition of diffusion conditions upon auto-catalysis which leads to the characteristic persulphate effect. This superposition of diffusion or coupling of diffusion with a chemical reaction in the photographic layer has been frequently noticed, and we can generalize the present result as follows:

1. If in a photographic process the reaction products affect

(accelerate or retard) the primary action, then the speed of this will depend upon their rate of removal and momentary concentration.

2. These products, if soluble, will tend to diffuse away most rapidly in the direction of regions of lower concentration.

3. From the known structure and distribution of the negative image, diffusion, and hence exhaustion, of reaction products will occur more easily in the lower densities than in the higher. Conversely, there will tend to be a superproportional accumulation of the reaction product in the more strongly exposed regions, the lower layers next to the support eventually acting as reservoirs.

Amounts of Iron.—The percentage quantity of iron involved is not of a very large order, as will be seen by the following data:

Sample	Make	Persulphate	Iron per gram Persulphate	Remarks
No. 1....	Commercial	98.3 per cent.	.0006 gram	Could just be used in in 5 per cent. solution. Quite active at once.
No. 2....	Commercial same firm different stock.	97.8 per cent.	.00162 gram	Could not be used in stronger than 2 per cent solution. Very active at once.
No. 3....	Laboratory product.	98.2 per cent.	.000088 gram	Very inactive; in 5 per cent. solution hardly any action in 15 mins. Iron, alum, or silver nitrate brought it up to No. 2.

In a later communication data on the relation of the iron concentration to the action measured sensitometrically will be given. Meanwhile it may be noted that in a 5 per cent. solution persulphate the amount of iron required to perceptibly affect the initial activity in dissolving silver is of the order of 1 part in 1,000,000.

The tolerance or upper limit of iron permissible appears to be of the order of that found in preparation (2), 1 to 2 parts per 1000 of solid persulphate.

My thanks are due to Mr. W. H. Davis for assistance in the experimental work.

Photochemical Activity of Ultra-violet Light. ANON. (*Metallurgical and Chemical Engineering*, vol. 18, No. 1, p. 43, January 1, 1918.)—The photochemical activity of invisible light-waves existing beyond the visible violet of the spectrum has long been a recognized fact, and considerable research has been made to determine the nature and extent of the reactions produced. The shortest wave-length of light perceptible to the eye is about 0.3969μ, in the violet end of the spectrum. Rays of shorter wave-length vary in their activity and effect. Berthelot divided them into four groups: (1) Wave-lengths 0.4 to 0.3μ, comprising the ultra-violet rays that reach the earth's surface in sunlight. These rays are similar to the violet rays of the visible spectrum. (2) Wave-lengths 0.3 to 0.2μ, medium rays of strong photochemical effect, producing marked chemical and physical changes. (3) Wave-lengths 0.2 to 0.15μ, useful in synthetic processes. (4) Wave-lengths 0.15 to 0.1μ, comprising an ultra-violet region which is little known.

Among the physical and chemical effects produced by ultra-violet light are many of practical value, affording quick tests for certain properties, and acceleration of certain chemical reactions. The sterilization of liquids such as water and milk, and of semi-solids like edible fats, is readily accomplished by ultra-violet rays. Exposure of only a fraction of a second under proper conditions is required for complete sterilization. Most dyes fade rapidly under treatment with ultra-violet light, and some remain fast. Fabrics like cotton, rubber, oil-cloth, etc., reveal in a comparatively few minutes the deterioration that otherwise would not become apparent for days in ordinary light. Numerous organic substances are bleached by its action.

Chemical reactions produced by ultra-violet light are many and varied. Chlorine derivatives of toluol are produced in greatly increased yields and without undesirable by-products if the reaction proceeds in the presence of ultra-violet rays. Other chlor-substitution organic products are formed with greater facility, in greater quantity, and of higher degree of purity if ultra-violet light enters into the reaction. The halogen elements generally seem sensitive to this force and enter into synthetic reactions rapidly under its influence. Cyanogen is converted into para-cyanogen, sulphur and phosphorus show allotropism, organic compounds undergo isomeric changes, hydrolytic reactions are accelerated, and numerous other changes occur under influence of ultra-violet rays.

The production of these rays in a manner suitable for application in the arts has been the subject of considerable invention. They are given off in concentrated form from a mercury-vapor lamp, but, since glass is impervious to them, it is necessary to construct the lamp of quartz if the rays are to be used.

An improved form of quartz mercury-vapor lamp which embodies new features in construction and application is manufactured by the R. U. V. Company, New York City. This lamp produces an ultra-violet component several times stronger than that yielded by other forms of quartz mercury-vapor lamps. The wave-lengths of its light have been measured as low as 0.18μ, indicating a high degree of activity according to Berthelot's classification given above.

NOTES FROM THE PHYSICAL LABORATORY OF THE UNITED GAS IMPROVEMENT COMPANY.*

REVISED FORMULA FOR LUMINOSITY CURVE SOLUTION FOR PHYSICAL PHOTOMETRY.

By Herbert E. Ives.

SINCE the publication of a luminosity curve solution formula by Ives and Kingsbury, a large number of measurements have been made by Crittenden and Richtmyer by the visual (flicker) photometric method taken as the criterion in the writer's physical photometric studies. The result of this work of Crittenden and Richtmyer is to lead to a slightly different choice of composition for the test color solutions, which over a "4-watt" lamp should measure equal by the average eye. The originally published and revised test colors are:

Original	Revised
$CuSO_4$...53 grams to 1 liter of water	$CuSO_4$...57 grams to 1 liter of water
$K_2Cr_2O_7$..72 grams to 1 liter of water	$K_2Cr_2O_7$..72 grams to 1 liter of water

This change corresponds to a change of two per cent. in the relative transmissions.

In order to meet this new test color composition the luminosity curve solution formula has been empirically changed to give the ratio unity in the physical photometer, as follows:

Original luminosity curve solution	Revised
$CuCl_2$ .....60 grams	$CuCl_2$ .....61.25 grams
$Co(NH_4)_2(SO_4)_2$ .....14.5 grams	$Co(NH_4)_2(SO_4)_2$.....14.5 grams
K_2CrO_4 ..... 1.9 grams	K_2CrO_4 ..... 1.9 grams
Water to..... 1 liter	Water to ..... 1 liter

The transmission of this new solution through the spectrum is probably very accurately represented by Kingsbury's formula, in which the values at the red end were purposely kept lower than the transmission of the original solution, the results of Crittenden and Richtmyer being known from correspondence. The

* Communicated by the Manager.

work was interrupted before spectrophotometric values could be obtained.

Occasion was taken to try the modification of this solution proposed by Coblentz and Emerson to represent the results of their determination of the equal energy spectrum luminosity curve. The value obtained with their solution for the ratio of yellow to blue transmission was 1.045 instead of unity. This may be explained by the fact that their determination of the luminosity curve was carried through, apparently for reasons of instrumental convenience only, at a much lower brightness than that used in the photometric study of the test colors by Crittenden and Richtmyer. Agreement of visual and physical methods of photometry is obviously dependent on the luminosity curve used in the latter being obtained under the conditions adopted for the former.

EXPERIMENTS ON THE BLACK BODY AT THE MELTING POINT OF PLATINUM AS A FIXED POINT IN PHOTOMETRY.

By Herbert E. Ives.

THESE experiments were intended to carry to practical realization the suggestions of Waidner and Burgess and the writer to utilize the melting point of platinum to establish a standard (not a unit) of luminous intensity. Attention was devoted to the effects of tension and thickness on the melting point, and to determining the best shape and dimensions for achieving the black body required.

It was found that lowering of the melting point, due to tension on the thin sheet metal employed, was insignificant for any tensions necessary to use in the suggested method of achieving the black body. No variation of melting point with thickness was found. These questions were tested by photometric criteria.

The acute-angled hollow wedge of sheet platinum used formerly was found much inferior as to uniformity of heating to a hollow cylinder of sheet metal, with a small slit opening parallel to the axis and viewed at an angle of about 20 degrees to the diameter passing through the opening.

This work was interrupted at the point where the practicability and precision of the method appeared to be established, but before data could be obtained with chemically pure platinum.

Using the same grade of platinum as that studied previously, whose melting point was determined as practically that of Hereaus platinum, the mean of several close determinations was 58.4 candlepower per square centimetre as the brightness of the black body at the melting point of platinum, in excellent agreement with the value obtained before (58.35).

This value, if used with Kingsbury's formula for the luminosity curve of the equal energy spectrum, and the value .00160 for the mechanical equivalent of light, is in harmony with the following selection of values for the physical constants involved.

$$\sigma = 5.72 \times 10^{-12}$$
$$C_2 = 14312$$
Melting point of platinum = 2037° K (1764° C)

It is hoped to continue this work at a later date.

December 31, 1917.

Adjusting Watches to Temperature. J. Thompson, Jr. (*The Horological Journal*, vol. 60, No. 714, p. 57, February, 1918.)—In the early days of watch- and chronometer-making, the compensation for changes of temperature was practically unthought of. If a person possessed a watch that would run and give him an approximate idea of the time of day, that was considered satisfactory and all that was necessary. As the methods of watchmaking advanced, certain watchmakers began to conceive of a plan to reduce the errors caused by temperature changes, and it remained for Harrison, a very clever English watchmaker, to devise one of the first means of compensation, consisting of a regulating arm made of brass and steel that would move with changes in temperature much the same as the compensating balances of the present day. The arm operated to move the regulator pins along the hairspring so as to slightly shorten the vibration in heat and lengthen them in cold, thus overcoming the effect of the changes in length of the spring.

This method was crude and unsatisfactory, even to the inventor, and was condemned by Julien le Roy, a noted French watch- and clock-maker, who abandoned the idea in favor of the compensating balance of brass and steel. These early balances were so satisfactory that they have undergone but slight changes in more than a hundred years. In any watch or chronometer, errors of time resulting from changes of temperature are caused by various other conditions than those of the balance and hairspring. Of these, the change in the fluidity of the oil is one of the most prominent, but the size of the balance as well as that of every other part of the watch is continu-

ally changing with the temperature. It is the office of the compensation to eliminate the errors of time from all these causes so far as it is possible to do so.

In the Waltham factory the watches are first run twenty-four hours at a temperature of 40° F., and the rate recorded. Next the watches are wound and reset, and after being allowed to run twenty-four hours at a temperature of 90° F. the rate is again recorded. From these rates the amount of adjustment of the screws on the balance wheel is estimated. After retiming, the test is a trial running and is not considered final even if the watch runs within the prescribed limits for its grade, as the balance does not attain its maximum compensation on first being exposed to extremes of temperature. The temperature adjustment is of extreme importance on account of its permanency, and with the same balance and hairspring need never be blamed for any but slight variations in the performance of a watch.

A New Method of Separating Materials of Different Specific Gravities. T. M. CHANCE. (*The Coal Age*, vol. 13, No. 19, p. 866, May 11, 1918.)—The separation of materials of different specific gravities by means of a fluid having a specific gravity greater than the lighter particles and less than that of the heavier particles has not been applied commercially or on a large scale to the separation of ores or to the washing of coal, the method being limited to laboratory experimental work or to laboratory determinations for the purpose of checking up the work of jigs, classifiers, and other types of concentrating appliances. A solution of zinc chloride has thus come into general use in the laboratory to separate coal, bony coal and slate for test purposes. Difficulties that are practically insuperable, however, have prevented the commercial development of the use of such solutions for making separations on a large scale, these difficulties being both physical and financial. The cost of the chemical used to make high-gravity solutions is usually prohibitive, and the freeing of the coal from all traces of the chemical is found to be practically impossible.

It has been found that a mechanical mixture of water and fine sand created and maintained by agitation will have sustention and therefore selection qualities similar to those of a real solution in which bodies having greater specific gravity will sink and those of less specific gravity will float. A mixture of sand and water can be used to produce a fluid mass having a specific gravity suitable for the separation of coal from its impurities. Fluid masses, having specific gravity high enough to float quartz, feldspar, limestone and other rocks, can be produced by using magnetic iron-ore sand and water instead of quartz sand and water. Other heavy materials can be used, such as ore concentrates of galena, metallic copper, etc., for producing fluid masses having very high specific gravities.

THE FRANKLIN INSTITUTE.

MEMBERS OF THE FRANKLIN INSTITUTE WHO ARE IN THE ACTIVE MILITARY OR NAVAL SERVICE OF THE UNITED STATES GOVERNMENT.

Name and rank	Branch of service	Location
Abbott, A. C., Lieut.-Col.	Greenleaf School of Military Hygiene	Chickamauga Park, Ga.
Allen, Henry B., 1st Lieut.	Ordnance Dept., U. S. R.	France
Anderson, Geo. L., Colonel	Board of Ordnance & Fortification	Washington
Atherholt, Gordon Meade	Aeronautical Mechanical Engineer, Signal Corps	Washington
Atterbury, W. W., Brig. Gen.	Director-General of Railways	France
Bacon, Raymond Foss, Lt. Col.	Head of Chemical Service Section, U.S.N.A.	France
Barnhart, G. Edw.	Aeronautical Mechanical Engineer, Signal Corps	Fairfield, Ohio
Barr, John H., Major	Ordnance Reserve Corps	Washington
Barrett, C. D., Major	9th Engineers	
Bartow, Edward, Major	U.S.N.A. Sanitary Corps	France
Booz, Horace Corey, Colonel	Railroad Transportation Corps, U.S.N.A.	France
Bostwick, John Vaughan, Capt.	O.R.C., 315th Infantry	Camp Meade
Breed, George, Lieutenant	Fleet Naval Reserve, U.S.N. R.F.	New York
Bunting, C. M., Colonel	E.O.R.C., U.S.A.	France
Cadwalader, Governeur, Major	Ordnance Dept., U.S.R.	
Caldwell, E. W.	Medical Officers' Reserve Corps	
Capps, W. L., Rear Admiral	Chief Constructor, U.S.N., Bureau of Construction and Repair	Washington
Carty, John J., Colonel	Signal Corps, U.S.A.	
Chance, Edwin M., Capt.	Ordnance Dept., U.S.R.	Washington
Clark, Beauvais, Sergeant	108th Field Artillery	Camp Hancock
Clark, E. L., 1st Lieut.	Signal Corps, 401st Telegraph Battalion	Camp Devens
Clark, Theobald F., Capt.	Field Artillery	
Clark, Walton, Jr., Capt.	Field Artillery	
Coates, Jesse, Major	American Expeditionary Force	France
Cornelius, John C., 1st Lieut.	Coast Artillery, U.S.N.A., 13th Company	Sandy Hook
Cottrell, Jas. W., Private	Instruction Section, Ordnance Dept., U.S.A.	Peoria, Ill.
Cowan, Henry B., Sergeant, 1st Class	Co. D., First Telegraph Battalion, Signal Reserve Corps	France
Crampton, George S., Major	Director of Field Hospitals, 28th Div.	Camp Hancock
Cushman, A. S., Lieut. Col.	Ordnance Dept., U.S.N.A.	Frankford Arsenal
Detwiler, Jas. G., 1st Lieut.	Infantry, U.S.R.	Fort Logan H. Roots

Name and rank	Branch of service	Location
Eckert, S. B., Lieut.	Commander 9th Aero Squadron	France
Edwards, John R., Rear Admiral	Brown University	Providence, R I.
Elliott, Henry M., 1st Lieut.	Ordnance Dept., U.S.R.	Western Cartridge Co.
Emerson, Geo. H., Colonel	In charge of Russian Railway Service Corps	Japan
Felton, Samuel M.	U. S. Director-General of Railways in connection with Expeditionary Force	Chicago
Ferguson, Walter B., Private	21st Co., 154th Depot Brigade	Camp Meade
Fraser, Persifor, Ensign	U.S.N.R.F.	League Island
Gardner, H. A., Senior Lieut.	Naval Flying Corps	Washington
Gfrörer, A. H., 1st Lieut.	Ordnance Dept., U.S.R.	Washington
Gibbons, Joseph E., Private	Co. D., 103rd Engineers	Camp Hancock
Gilbreth, Frank B., Major	Engineers O. R. C.	Washington
Gillmor, R. E., Senior Lieut.	U. S. Navy	New York
Glendinning, Robt. E., Major	Aviation Section, Signal Corps	Overseas
Goodwin, Harold, Jr., Lieut.	Naval Reserve Force	Philadelphia
Gribbel, W. G., Captain	Co. A., 30th Engineers, U.S.R. (Gas and Flame)	France
Griest, Thos. S., 1st Lieut.	1st Telegraph Battalion, Signal Corps, U.S.A.	France
Hall, R. T., Rear Admiral	U. S. Navy, Inspector of Machinery	Cramps' Shipyard
Hodges, Austin L., Capt.	Ordnance Dept., U.S.R., Experimental Officer on Artillery Ammunition	Frankford Arsenal
Howson, Richard, Sergeant	306th Ambulance Corps	Camp Meade
Ives, H. E., Captain	Signal Corps, U.S.A.	Washington
Jackson, John Price, Major		France
Jones, Jonathan, Captain	E.O.R.C., 23rd Engineers	Camp Laurel
Karrer, Enoch, Private	437th Engineers	Washington
Kennedy, M. C., Colonel	Deputy Director General of Transportation	France
Kent, S. Leonard, Jr., 2nd Lieut.	5th Engineers	Camp Lee
Kingsbury, E. F., 1st Lieut.	Aviation Section, S.O.R.C., Dept. of Science and Research	Washington
LeBoutillier, H. W., Private	Unit No. 10, Pennsylvania Hospital	France
Lichtenberg, Chester, 1st Lieut.	Engineer Reserve Corps, U.S.A.	Washington
Longstreth, Chas., Lt. Commander	U.S.N.R.F.	Philadelphia
McCoy, John F.	Aviation Section, Signal Corps	Princeton
MacLean, Malcolm R., Major	Infantry R C.	Camp Meade
McMeekin, C. W., Major	Army War College	Washington
Martin, Thos. S., 1st Lieut.	Ordnance Dept., U.S.R.	Washington
Masters, Frank M., Major	Ordnance Dept., U.S.R.	Washington
Maxfield, H. H., Lieut. Col.	19th Railway Engineers, U.S.N.A.	
Mershon, Ralph D., Major	E.O.R.C., U.S.A.	New York
Miller, Fred. J., Major	Ordnance Reserve Corps	Centre Bridge, Penna.
Owens, R. B., Major	Chief Signal Officer, Base Section No. 3, A.E.F.	London
Parrish, T. R., Captain	Signal Corps	Washington
Reber, Samuel, Colonel	Signal Corps, U.S.A.	New York
Richardson, Chas. E., 1st Lieut.	30th Engineers, U.S.R. (Gas and Flame)	France

Name and rank	Branch of service	Location
Spackman, Henry S., Major	Engineer Officers' Reserve Corps	France
Spruance, W. C., Jr., Lt. Col.	Ordnance Dept., National Army	Chevy Chase, Md.
Squier, Geo. O., Major General	Chief Signal Officer, U.S.A.	
Stanford, H. R., Civil Engineer	U. S. Navy	Boston
Thomas, Geo. C., Jr., Captain	Aero Service Squadron 96, Aviation Section, Signal Corps	France
Tilghman, B. C., Captain	A.D.C., 28th Div. Headquarters, U.S.A.	
Vogleson, J. A., Major		Camp Jas. E. Johnston
Wagner, Fred. H., Major	Ordnance Reserve Corps, Nitrate Division	Washington
Wagner, Fred. H., Jr. Sergeant	Co. E., 304th Engineers	Camp Meade
Wells, G. A., Captain	Ordnance Reserve Corps	Peoria, Ill.
Wetherill, W. C., Ensign	U. S. Navy	
Widdicombe, R. A., Major	C.Q.M., Chemical Plant No. 4	Saltville, Va.
Wood, Edw. R., Jr., Captain	18th Field Artillery, U.S.N.A.	El Paso, Texas
Worrell, Howard Sellers	3rd Officers' Training Camp	Fortress Monroe
Wyckoff, A. B., Lieut.	U. S. Navy	Ontario, Cal.
Yale, A. W. Major	Medical Reserve Corps, Gas Division	Camp Kearney
Yorke, George M., Major	Signal Corps, U.S.R.	New York

MEMBERS DOING CIVILIAN WORK FOR THE UNITED STATES GOVERNMENT

Name	Appointment	Location
Akeley, Carl E.	Consulting Expert of Mechanical Devices, War Department	Washington
Anderson, Robt. J.	Aeronautical Mechanical Engineer on Metallography, Bureau of Aircraft Production	Pittsburgh, Pa.
Balls, William H.	Ships Draughtsman	Phila. Navy Yard
Bancroft, Joseph	Secretary, Local Board No. 1	Wilmington
Baskerville, Charles	Working with Bureau of Mines, Ordnance Dept. (Gas Warfare, Shells, etc.)	New York
Bodine, Samuel T.	Vice-Chairman, District Exemption Board, No. 1, Eastern Judicial District of Penna.	Philadelphia
Brown, Lucius P.	Federal Milk Commission	New York
Burnham, George, Jr.	Dept. of Civilian Service and Relief, Public Safety Committee of Pennsylvania	Philadelphia
Charles, Bernard S.	Ordnance Inspector, U. S. Navy	Allentown, Pa
Comey, Arthur M.	Chairman, Sub-Committee on Explosives, Chemistry Committee, National Research Council	Chester, Pa.
Condict, G. Herbert	Naval Consulting Board, Member Committee of Examiners	Plainfield, N. J.
Cooke, Morris L.	Chairman, Storage Committee of Munitions Board; Member on Staff, Emergency Fleet Corporation	Washington
Cope, Thomas D.	National Research Council	Washington

Name	Appointment	Location
Day, Charles	Member of Army War Council	Washington
Delano, Frederic A.	Member of Federal Reserve Board	Washington
Dickie, G. W.	Chief Inspector, U. S. Shipping Board, Moore & Scott Shipyard	Oakland, Calif.
Dunn, Gano	Chairman, Engineering Committee, National Research Council	New York
Garrison, Frank Lynwood	Chairman, U. S. Manganese Commission	Philadelphia
Halberstadt, Baird	Federal Fuel Administrator for Schuylkill Co., Penna.	Pottsville
Henderson, George R.	State Advisory Engineer, Federal Fuel Administration for Pennsylvania	Philadelphia
Hornor H. A.	Electrical expert for Industrial Training, Emergency Fleet Corporation	Philadelphia
Hoskins, Wm.	Consulting Chemist, Advisory Committee, Bureau of Mines; Associate Member, Naval Consulting Board	Chicago
Hyde, Edward P.	National Research Council, Sub-Committee on Monocular vs. Binocular Field-Glasses (Chairman).	Cleveland
Insull, Samuel	Chairman, Illinois State Council of Defense	Chicago
Kennelly, A. E.	Conducting special course in radio-engineering for the U. S. Signal Corps, in conjunction with Prof. E. C. Chaffee	Harvard Univ.
Lenher, Victor, Major	Chemical Service Section, N. A.	Washington
Lloyd, E. W.	Asst. Secretary, Illinois State Council of Defense	Chicago
Lucke, C. E.	Civilian Director, U. S. Navy Gas Engine School	Columbia Univ.
Merrick, J. Hartley	Director, Bureau of Camp Service, Penna. Div., American Red Cross	Philadelphia
Milne, David	Treasurer, American Red Cross General Hospital No. 1	Philadelphia
Morris, Effingham B.	Treasurer, Committee of Public Safety, State of Pennsylvania	Philadelphia
Nichols, Carroll, B.	Fuel Administration	Washington
Nichols, Wm. H.	Committee on Chemicals, Advisory Council of National Defense, Consulting Chemist, Bureau of Mines	New York
Penrose, R. A. F., Jr.	Member of Geology Committee of the National Research Council	Philadelphia
Rapp, Isaiah M.	Special Investigator of Weights and Measures for the U. S. Food Administration	Norman, Oklahoma
Rautenstrauch, Walter	Committee of the National Research Council	New York
Richards, Joseph W.	Member of Naval Consulting Board	So. Bethlehem, Penna.
Robins, Thomas	Member and Secretary of the Naval Consulting Board	New York
Sauveur, Albert	Director of Research, Division of Metallurgy, Technical Air Service, A.E.F.	France
Sperry, Elmer A.	Member of Naval Consulting Board	Brooklyn, N.Y.

Name	Appointment	Location
Sprague, Frank J.	Member of Naval Consulting Board, Chairman, Committee on Electricity and Shipbuilding	
Stern, Max J.	Supervising Surgeon, Merchant Shipbuilding Corporation	Philadelphia
Stradling, George F.	Recruiting for Aviation Section, S.O.R.C.	Philadelphia
Swenson Magnus	Federal Food Administrator for Wisconsin; Chairman, State Council for Defense	Madison, Wis.
Talbot, Henry P.	Member of Advisory Board, Bureau of Mines (Gas Defense)	Cambridge, Mass.
Thomson, Elihu	National Research Council in coöperation with Council of National Defense	Swampscott, Mass.
Wadleigh, Francis R.	Emergency Fleet Corporation	Philadelphia
Warwick, J. F.	Bethlehem Loading Co.	Mays Landing, N. J.

Please send additional information and corrections to the Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(Abstract of Proceedings of the Stated Meeting held Wednesday, June 5, 1918.)

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, June 5, 1918.

DR. H. JERMAIN CREIGHTON *in the Chair.*

The following reports were presented for final action:

No. 2708.—The Waterbury Hydraulic Speed Gear. Recommended that the Elliott Cresson Medal be awarded, jointly, to Reynold Janney, of New York, N. Y., and Harvey D. Williams, of Washington, D. C., for the invention of The Waterbury Hydraulic Speed Gear.

No. 2714.—H. C. Snook's Improved X-Ray System. Report referred back to Sub-Committee for further consideration.

GEORGE A. HOADLEY,
Acting Secretary.

MEMBERSHIP NOTES.

ELECTIONS TO MEMBERSHIP.

(Stated Meeting Board of Managers, June 12, 1918)

NON-RESIDENT

MR. JAMES A. FLYNN, Production Engineer, 112 Brilliant Avenue, Aspinwall, Pa.

CHANGES OF ADDRESS.

MR. G. EDWARD BARNHART, 413 West Jersey Street, Elizabeth, N. J.

MR. CALVIN BARWIS, in care Pennsylvania Railroad Company, Warren, Pa.

REAR ADMIRAL JOHN R. EDWARDS, U.S.N., Brown University, Providence, R. I.

PRIVATE ENOCH KARRER, 437 Engineers, in care Bureau of Standards, Washington, D. C.

MAJOR VICTOR LENHER, Chemical Service Section, N.A., War Department, 7th and B Streets, N.W., Washington, D. C.

MR. JOHN F. MCALLISTER, Hotel Vendig, Philadelphia, Pa.

MR. I. F. STONE, 21 Burling Slip, New York, N. Y.
MR. FRANCIS R. WADLEIGH, Emergency Fleet Corporation, 1528 Chestnut St., Philadelphia, Pa.
MR. J. F. WARWICK, in care Bethlehem Loading Company, May's Landing, N. J.

LIBRARY NOTES.

PURCHASES.

American Institute of Chemical Engineers. Transactions, vol. 9. 1916.
Boyd's Philadelphia Blue Book. 1918.
California State Mining Bureau. Annual Reports. No. 1 and No. 4. 1880, 1884.
CAVEN, R. M.—Carbon and Its Allies. 1917.
Engineering Index Annual, vol. 34. 1917.
FRAPS, G. S.—Principles of Agricultural Chemistry. 1917.
KETCHUM, M. S.—Design of Steel Buildings, and the Calculation of Stresses in Framed Structures. 1912.
KETCHUM, M. S.—Structural Engineers' Handbook. 1914.
LECORNU, LEON.—Cours de Mecanique, vol. 1, 1914. Vol. 3, 1918.
MCADIE, ALEXANDER.—Principles of Aërography. 1917.
MCSHANE, C. L.—Modern Locomotive Valves, and Valve Gears. 1917.
PARKINSON, JAMES.—Chemical Pocket Book; or Memoranda Chemica. 1802.
ROEBUCK, J. R.—Science and Practice of Photography. 1918.
ROGERS, ALLEN.—Laboratory Guide of Industrial Chemistry. 1917.
Society of Chemical Industry, Reports of the Progress of Applied Chemistry, vol. 1. 1916.
Who's Who; An Annual Biographical Dictionary. London. 1918.
WILEY, H. W.—Food and Its Adulteration. 1917.

BOOK NOTICES.

COURS DE MÉCANIQUE PROFESSÉ A L'ÉCOLE POLYTECHNIQUE, par Léon Lecornu, Membre de l'Institut, Inspecteur Général des Mines. In three volumes, 8vo, paper, sold separately. Volume I, vii + 536 pages with 281 illustrations, 1914, price, 18 francs; volume II, vi + 538 pages with 110 illustrations, 1915, price, 18 francs; volume III, vi + 667 pages with 211 illustrations, 1918, price, 25 francs. Paris, Gauthier-Villars et Cie.

Subordinate only to adequate training in mathematics, a thorough knowledge of analytical mechanics is the essential basis of a study of the physical sciences and their applications in various branches of technology, and a text on this important fundamental should embody a supreme effort of professional and pedagogical skill. As the author points out, while laying stress on the theorems of pure mechanics, it should be designed to develop an appreciation of material values, and, to a certain extent, serve as an introduction to the particular applications in which the student may specialize.

Addressed to students of a school representative of the highest standard of scholarships, Professor Lecornu's work is planned on these broad lines. It presents the subject on the solid foundation of the development of basic theorems at considerably greater length than is usually deemed needful in many of our engineering courses, and is of a type suitable to the investigator as well as to the analytical practitioner.

The first two volumes cover kinematics, statics, dynamics, elasticity, and the mechanics of fluids. The third volume is devoted to the applications, resistance of materials, hydraulics, thermodynamics, theory of machine, and the elements of aviation. These divisions of the subject, the author states, are not now included in the course in mechanics of the École Polytechnique, but have been added in anticipation of radical changes that may be expected in technical education to meet the industrial competition of post-war conditions, when the attainment of material accomplishment rather than scholarly attainment will have become imperative. The work constitutes essentially the theoretical part of a course in mechanical engineering in which the study of fundamental principles has been dwelt upon as exhaustively as practicable limitations of time permit.

Although intended as a text, a work covering such an extensive field should prove valuable for reference and for that reason the omission of an index is to be regretted. However, very full tables of contents partly compensate for this omission. As is usual with the products of its widely known publishers, the typographical features of the work are of a high order of excellence. LUCIEN E. PICOLET.

THE CHEMICAL ANALYSIS OF IRON: A COMPLETE ACCOUNT OF ALL THE BEST KNOWN METHODS FOR THE ANALYSIS OF IRON, STEEL, PIG-IRON, ALLOY METALS, IRON-ORE, LIMESTONE, SLAG, CLAY, SAND, COAL AND COKE. By Andrew Alexander Blair. Philadelphia, J. B. Lippincott Company, 1918. Eighth edition, 318 pages, illustrations. 8vo. Price, $5.00.

Since the first edition of this work appeared, about 30 years ago, it has held a conspicuous place as *the Standard*, both in colleges and in industrial works. The fact that it has now passed through no less than eight editions is prima facie evidence that it has kept pace with the march of progress and a truly wonderful march this has been!

"Alloy Steels" were unborn when the earlier editions appeared. Tungsten, Molybdenum, Vanadium, etc., were rare elements found only, if at all, in minute specimens in cabinets of minerals and metals. All of these now constitute very important alloys with iron and are classed under the generic name of Alloy Steels. There were no standard methods of analysis of these alloys recorded in any one book so far as the reviewer knows, before they appeared in the work under discussion. Even fifty per cent. ferro-silicon, now so familiar to iron and steel metallurgists, was not produced practically until after the development of electric furnaces and it is within the memory of living men when eighty per cent. ferro-manganese was a novelty. Some of these alloys are insoluble in the usual acid solvents for iron compounds, hence entirely different methods of preliminary treatment of samples for analysis were required. Various original methods were devised by different investigators toiling in their laboratories and gradually formulæ for rapid determinations were evolved for use in daily work in industrial establishments.

Examination of the eighth edition of "The Chemical Analysis of Iron" reveals not only the methods that the writer has already seen in print, but others which are entirely new to him. He therefore has no hesitation in pronouncing this work thoroughly up to date. A. E. OUTERBRIDGE, JR.

PUBLICATIONS RECEIVED.

How to Become a Wireless Operator: A Practical Presentation of the Theory of Electrical Waves, Their Propagation, and Their Adaptation to Wireless Communication, Including Simple and Clear Instructions How to Operate Wireless Devices and How to Comply with Government Requirements for Operators, by Charles B. Hayward. 312 pages, illustrations, 8vo. Chicago, American Technical Society, 1918.

Machine Design: A Manual of Practical Instruction in Designing Machinery for Specific Purposes, Including Specifications for Belts, Screws, Pins, Gears, etc., and Many Working Hints as to Operation and Care of Machines, by Ernest L. Wallace, B.S., E.E. 164 pages, illustrations, 8vo. Chicago, American Technical Society, 1918.

Portland Cement Association: Catalogue of Books, Periodicals and Pamphlets in the Library. 59 pages, 8vo. Chicago, Association, 1918.

Athenæum Subject Index to Periodicals, 1916: Issued at the Request of the Council of the Library Association. Science and Technology, Including Hygiene and Sport. 162 pages, quarto. London, The Athenæum, 1918. Price, 10 shillings net.

U. S. Coast and Geodetic Survey: Results of Observations Made at the United States Coast and Geodetic Survey Magnetic Observatory, Near Tucson, Arizona, 1915 and 1916, by Daniel L. Hazard, Chief, Division of Terrestrial Magnetism. 101 pages, plates, quarto. Washington, Government Printing office, 1918. Price, 25 cents.

Successful Trial Trip of the Reinforced Concrete Cargo Steamship Faith. 15 pages, illustrations, 8vo. Chicago, Portland Cement Association, 1918.

U. S. Department of Agriculture: Bulletin No. 606. Professional Paper. Relative Resistance of Various Hardwoods to Injection with Creosote, by Clyde H. Teesdale, in charge, Section of Wood Preservation, and J. D. Maclean, Assistant Engineer in Forest Products. 36 pages, illustrations, plates, 8vo. Washington, Government Printing Office, 1918.

U. S. Bureau of Mines: Bulletin 149. Bibliography of Petroleum and Allied Substances, 1915, by E. H. Burroughs. 147 pages, 8vo. Monthly Statement of Coal-mine Fatalities in the United States, February, 1918. List of Permissible Explosives, Lamps, and Motors Tested Prior to March 31, 1918, compiled by Albert H. Fay, 21 pages, 8vo. Technical Paper 162: Initial Priming Substances for High Explosives, by Guy B. Taylor and W. C. Cope. 32 pages, 8vo. Technical Paper 193: Quarry Accidents in the United States During the Calendar Year 1916, compiled by Albert H. Fay. 58 pages, 8vo. Technical Paper 202: Metal-mine Accidents in the United States During the Calendar Year 1916. Compiled by Albert H. Fay. 91 pages, 8vo. Washington, Government Printing Office, 1918.

PRESS OF
J. B. LIPPINCOTT COMPANY
PHILADELPHIA